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THE WEIGHT AND GROWTH OF THE HUMAN EYEBALL

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IN 1927 at the request of Professor Henry H. Donaldson we undertook to obtain data on the size of the human eyeball in adult life and its growth during childhood. The unique facilities of Western Reserve University and the Hamann Museum provided an excellent opportunity to prosecute this work. It could not be carried out on the extensive basis originally planned, however, owing to the necessarily slow progress in collection of suitable material which entailed the training of successive workers and thus resulted in discontinuity.

We have therefore decided to assemble the results of the data obtained on 215 pairs of eyeballs from individuals ranging from birth to adult age. Sixty-nine pairs have been used in a special study of growth. Four pairs of these, from individuals of more than seventeen years of age, are also included in the adult series of 150 pairs.

The eyeballs were taken from cadavers which had been embalmed with a ten percent formalin solution. Naturally the fluid content of the eyeballs had diminished after death and the true weight was no longer obtainable. Since, however, the specific gravity of vitreous humor is

TABLE 1

Restoration of living weight of eyeball by soaking and distension

CADAVER NUMBER	FRESHLY REMOVED	SOAKED WATER 24 HRS.	SOAKED 5% FORMALIN 24 HRS.	FIRST DISTENTION	SECOND DISTENSION
	grams	*grams*	*grams*	*grams*	*grams*
1546 R	5.32	6.00	6.27	7.33	7.39
1546 L	5.19	5.90	6.00	7.08	7.30
1547 R	6.74	6.40	6.67	8.45	8.58
1547 L	5.64	6.30	6.65	7.67	8.12

close to that of water and the specific gravity of aqueous humor practically that of water (Vierordt, 1906) we distended the eyeballs to capacity through the needle of a 10 c.c. syringe. The needle was obliquely directed through the sclerotic after all muscular and connective tissues had been carefully and completely removed. High degree precision is impossible by the nature of the material and accuracy to one-tenth of a gram was accepted on successive weighings. It was found that the sclerotic resists over-distension of the eyeball with water and successive experiments on distension and weighing produced iden-

TABLE 2

Restoration of living weight by distension of eyeball

CADAVER NUMBER	BEFORE DISTENTION	AFTER DISTENTION	CADAVER NUMBER	BEFORE DISTENTION	AFTER DISTENTION
	grams	*grams*		*grams*	*grams*
818 R	1.00	3.51	1739 R	5.00	7.68
818 L	0.90	3.83	1739 L	3.00	7.40
1589 R	5.00	5.67	1747 R	5.50	6.48
1589 L	6.15	6.75	1747 L	4.25	6.30
1607 R	3.20	8.20	1750 R	5.55	6.07
1607 L	4.10	6.82	1750 L	5.57	6.20
1702 R	7.20	8.35	1753 R	5.15	6.33
1702 L	3.52	8.00	1753 L	6.07	6.40
1708 R	6.20	6.60	1755 R	6.40	7.70
1708 L	6.35	6.58	1755 L	5.97	7.80

tical results within the limit of precision set forth. The full data on all experiments are filed in the laboratory for future reference. Condensed results only are included in this presentation. Leaking of water from the distended eyeball is slow enough to permit careful drying of the exterior with a cloth before weighing. Experiments conducted to ascertain the immediate loss from leaks showed that this source of error is negligible. Our first experiments demonstrate the unreliability of dead eyeballs, even when soaked in water, to give the approximate living weight. Tables 1 and 2 show that only after a second distension can the dead eyeball weight be relied upon as a guide to living weight. The

TABLE 3

Experiments on formalin fixation. Weights of eyeballs distended before and after immersion in 5 percent formalin

CADAVER NUMBER		BEFORE	AFTER	DURATION	AVERAGE INCREASE
		grams	*grams*	*hours*	*per cent*
1546	R	7.33	7.39	24	
	L	7.08	7.30		
1547	R	8.45	8.58		
	L	7.67	8.12		2.81
1545	R	7.03	7.09	48	
	L	7.04	7.05		
1548	R	7.90	8.05		
	L	7.90	8.00		
1549	R	7.74	7.82		
	L	7.92	7.97		
1554	R	6.70	6.95		
	L	6.77	6.80		
1560	R	7.34	7.47		
	L	7.42	7.52		1.30
1551	R	8.09	8.07	72	
	L	7.70	7.78		
1555	R	7.88	7.94		
	L	7.75	7.86		
1556	R	7.69	7.72		
	L	7.50	7.58		
1558	R	7.10	7.41		
	L	7.07	7.29		1.43

TABLE 4

Influence of preservation in 2 percent formalin for three weeks. Weights of eyeballs distended before and after the 3 week interval

CADAVER NUMBER	BEFORE		AFTER
	First	Second	Third
	grams	*grams*	*grams*
1627 R	8.80	9.00	8.92
1627 L	8.90	9.05	9.00
1631 R	9.90	9.90	9.60
1631 L	9.90	9.90	9.60

limits of precision are clearly indicated in the final columns of Table 1. It is to be understood that all final figures adopted in this communication are those of the second distension.

We then ascertained the effect of preservation in formalin on eyeball size and found that, provided distension is carried out within a reasonable time after removal of the eyeball, preservation in formalin

TABLE 5

Weights of eyeballs distended before and after immersion in 2 percent formalin for 3 weeks

CADAVER NUMBER	BEFORE	AFTER	CADAVER NUMBER	BEFORE	AFTER
	grams	*grams*		*grams*	*grams*
1297 R	6.50	6.50	1525 R	7.40	7.40
L	6.50	6.50	L	7.40	7.40
1332 R	8.30	8.30	1562 R	7.15	7.15
L	8.30	8.30	L	7.15	7.15
1358 R	7.30	7.30	1610 R	7.60	7.60
L	6.90	7.30	L	7.60	7.60
1474 R	7.45	7.50	1612 R	6.98	7.12
L	7.45	7.50	L	6.98	7.12
1514 R	8.00	8.00	1615 R	8.48	8.50
L	8.00	8.00	L	8.48	8.50
1519 R	7.40	7.40	AVE. INCREASES 0.49%		
L	7.40	7.40			

is not prejudicial (Table 3). Hence experiments were conducted with 2 percent formalin. These indicate that immersion of the eyeball in weak formalin does not modify its distensibility (Tables 4 and 5).

The work on adult eyeballs was performed successively by Beecher and Williams, both of whom made some observations on children's eyeballs. The bulk of the observations and the statistical reductions were carried out by the younger Todd and the assembly of the data made by the senior author.

Table 6 gives the relevant data on adult eyeball weights, mean ages, statures and cranial capacities of the 150 adults examined in this study. In absolute figures the right eyeball is the larger in both white and Negro males but since there is obviously no difference between right and left eyeballs of the females in either series, this distinction is better not emphasized.

TABLE 6

Weight of adult eyeball 17 years and upwards

Male whites	63
Male Negroes	45
Female whites	18
Female Negroes	24
Total cadavers	150

	MALE WHITES	MALE NEGROES	FEMALE WHITES	FEMALE NEGROES
Mean age (*years*)	52	37	64	39
Right eyeball weight:				
Mean	7.85±0.05	8.51±0.10	7.29±0.10	7.95±0.11
Standard deviation	.60±0.04	1.00±0.07	.65±0.07	.80±0.08
Coefficient of variation	7.64±0.46	11.75±0.85	8.92±1.01	10.06±0.99
Left eyeball weight:				
Mean	7.82±0.05	8.47±0.10	7.28±0.10	7.94±0.11
Standard deviation	.60±0.04	.97±0.07	.65±0.07	.78±0.08
Coefficient of variation	7.67±0.46	11.45±0.82	8.93±1.01	9.82±0.96
Mean cranial capacity (c.c.)	1445	1393	1261	1273
Mean stature (mm.)	1734	1742	1590	1669

The coefficient of correlation is negligible
(Cr. cap.: R eyeball wgt. for male white, $r = -.0664 \pm .0849$)

Leopold Weiss in 1897 gave an excellent summary of work done up to that date commencing with Sömmering's systematic study in 1844. It is true of course that Meckel in 1820 had made a few desultory observations on eyeball growth and Krause in 1832 had actually weighed eyeballs. The uncertainty concerning all the work summarized in Weiss's paper is the technical detail of preparation. Judging by our own results it is doubtful if the earlier observers distended the eyeballs: they seem to have relied on obtaining them fresh. This is a vain hope in view of the loss of fluid and collapse of the eyeball soon after death. Although our quite laborious investigation gives additional data of racial significance it does not throw any really new light on the weight of the adult eyeball in white subjects except to give a weight more closely approximating that characteristic of the living.

The method of selecting eyeballs which show no evidence of shrinkage or distortion was applied by Scammon and Armstrong to the fetus (1925), but again one cannot be sure that the weights are really characteristic of the living fetus though they will approximate it more nearly than when the method is applied to eyeballs of the post-natal period. To this study we shall refer later in our presentation.

Body weight is not included in any of our tabulations because the dead weight of a human cadaver is unreliable as a criterion of the living weight in health (Todd and Lindala, 1928a, p. 42; 1928b, p. 154).

Stature was measured by the method described in an earlier communication (Todd and Lindala, 1928a, p. 40). The standards used for stature up to birth length (501 mm. male, 495 mm. female) are those of Scammon and Calkins (1929); for stature beyond this, Simmons and Todd (1938). Cranial capacity was measured by filling the two halves of the cranial cavity in the bisected skull with Climax wallpaper cleaner according to a technique to be later described (Todd and others: in preparation). This method is used in preference to the water method because of its apparently greater uniformity of result. The cranial capacity of the dried skull is practically the equivalent of the cranial capacity of the green skull before removal of the membranes (Todd, 1923, p. 129) and is about 300 c.c. greater than actual brain volume (Todd, 1933a, p. 110-112).

We have no explanation of the relatively small variability in weight of the adult male white eyeball but it is evident that this is not a function of the number measured because there is no appreciable difference between females of the two racial stocks or between male and female Negroes despite the difference in size of the respective samples.

Taking the crude averages we find the following relationships in weight of eyeballs by sex and stock:

$\frac{\text{Male white}}{\text{Female white}}$	1.075
$\frac{\text{Male white}}{\text{Male Negro}}$	0.923
$\frac{\text{Male white}}{\text{Female Negro}}$	0.987
$\frac{\text{Female white}}{\text{Female Negro}}$	0.917

These figures will later be used in constructing the charts of eyeball growth.

Brain volume and weight in the cadaver are not dependable (Donaldson, 1894; Hrdlička, 1906). Hence the coefficient of correlation, in male whites, was calculated for eyeball weight with cranial capacity which can be legitimately used as an indicator for size of the brain (von Bonin, 1934, p. 4). The data give $r = -0.0664 \pm 0.0849$. In the face of this discouraging result no computation was made for the females or for the Negro males.

It is likewise apparent that no useful result would be obtained from a correlation of eyeball weight with stature, and none was possible for eyeball weight with body weight. It is to be noted that Weiss and Scammon and Armstrong did lay out a comparison between increase of eyeball weight and of body weight but their results merely give the general evidence that growth of the eyeball follows that of brain more closely than that of body. Our correlation emphasizes the quite general character of this similarity of growth pattern. It is applicable only to mass statistics and not to individuals.

From our table it is evident that the Negro eyeball is larger than the white and the female eyeball is relatively large for body size.

A random series of 19 pairs of eyeballs was selected for special investigation of tissue weight. These eyeballs were distended, weighed, bisected, squeezed free of fluid and weighed again. No tissue was removed and the lens was not extruded. Results show that there are rare instances when one eyeball is actually larger than the other. Except in these, such differences as exist in actual eyeball tissue weights on

right and left sides fall within the unavoidable error of determination (Table 7).

For completeness of presentation, Table 8 has been constructed to show the evidence on eyeball weight by decades. Scanty as the data necessarily must be the results lend no color to any suggestion that a real change can be demonstrated in size of the eyeball after adult dimensions are reached. That they are actually reached in early adulthood is evident from the data shortly to be set forth (Table 9). There is moreover a wide fluctuation in the relationship of eyeball weight to cranial capacity. While eyeballs must be considered an outlying portion of the brain, the tissue of non-neurological origin composing the bulk of the eyeball obscures the relationship which is so distinctly shown in Jackson's experiments on the recovery of organ growth in the rat after starvation. It will be recalled that (despite refeeding after starvation

TABLE 7

Difference between right and left eyeball weights in grams

CADAVER NUMBER	DISTENDED	SOLID TISSUES	FLUID CONTENTS
1686	0.2	—0.6*	0.8
1729	0.9	0.0	—0.9
1834	0.2	0.5	—0.3
1922	0.2	0.2	0.0
1923	0.0	1.8	—1.8
2023	0.2	0.1	0.1
2036	0.55	0.30	0.25
2075	0.3	—0.3	0.6
2081	0.3	0.0	0.3
2118	0.4	0.0	0.4
2119	0.5	0.7	—0.2
2124	0.2	—0.1	0.3
2134	0.2	0.2	0.0
2149	0.4	0.0	—0.4
2183	0.7	0.3	0.4
2200	0.4	0.2	0.2
2233	0.3	0.3	0.0
2238	0.2	0.1	0.1
2240	0.8	0.5	0.3

* The minus sign means that left is greater than right.

TABLE 8

Average weights of eyeballs by decades

(Data extracted from adult series)
20 to 79 years

	MALE WHITES	MALE NEGROES	FEMALE WHITES	FEMALE NEGROES
20-29	2	8	0	5
Av. age (*years*)	29	23	..	25
Av. stature (*mm.*)	1671	1761	..	1681
Av. cr. cap. (*c.c.*)	1541	1379	..	1233
Right eye (*gms.*)	7.28	8.95	..	7.64
Left eye (*gms.*)	7.28	8.97	..	7.61
30-39	8	17	1	8
Av. age (*years*)	36	35	33	35
Av. stature (*mm.*)	1708	1747	1612	1666
Av. cr. cap. (*c.c.*)	1447	1380	1070	1314
Right eye (*gms.*)	7.75	8.51	7.83	7.89
Left eye (*gms.*)	7.73	8.45	7.93	7.66
40-49	14	11	4	6
Av. age (*years*)	44	43	45	46
Av. stature (*mm.*)	1765	1762	1632	1660
Av. cr. cap. (*c.c.*)	1437	1460	1437	1238
Right eye (*gms.*)	7.89	8.48	7.19	7.69
Left eye (*gms.*)	7.84	8.40	7.06	7.64
50-59	20	3	2	1
Av. age (*years*)	54	55	54	50
Av. stature (*mm.*)	1740	1733	1655	1751
Av. cr. cap. (*c.c.*)	1509	1423	1268	1466
Right eye (*gms.*)	8.08	8.12	7.76	9.22
Left eye (*gms.*)	8.06	8.17	7.79	9.17
60-69	13	3	2	2
Av. age (*years*)	65	62	63	60
Av. stature (*mm.*)	1710	1677	1589	1613
Av. cr. cap. (c.c.)	1361	1318	1378	1196
Right eye (*gms.*)	7.84	8.38	7.20	8.06
Left eye (*gms.*)	7.83	8.28	7.15	7.98
70-79	6	0	7	1
Av. age (*years*)	73	..	75	71
Av. stature (*mm.*)	1747	..	1560	1729
Av. cr. cap. (*c.c.*)	1398	..	1278	1319
Right eye (*gms.*)	7.35	..	7.39	10.00
Left eye (*gms.*)	7.37	..	7.43	10.20

from birth) brain, and from three weeks after birth, eyeballs, fail to reach the weight levels ultimately characteristic of the controls (Jackson and Stewart, 1919, 1920; Jackson, 1925, 1936). That the destiny of eyeball growth is linked with brain growth there is no doubt, but our data give no relevant information on the subject.

The 69 pairs of eyeballs included in the growth series require consideration from a different point of view. They range in age from one day to 21 years but the great majority belong to the first lustrum of five years or under. Table 9 gives average eyeball weight, corrected weight in terms of the male white, chronological age and skeletal age. Skeletal age is computed upon the bony penetration of cartilages of carpus, tarsus and epiphyses by the methods worked out in this laboratory and previously published (Todd, 1932, 1933b, 1937). Since these publications of our methods, however, the methods themselves have been elaborated to provide a finer objective discrimination of progress in maturity and have been extended to cover the life-span between the third fetal month and early adulthood (Hill, 1939; Francis and Werle, 1939). The importance of skeletal age is that it indicates the stage of bodily maturity as distinct from bodily growth in dimension, on which it forms a check. Bodily growth is subject to either retardation or acceleration whereas maturation progress is relatively stable. In the bodies of dead children who have died of disease terminating a period of ill health the skeletal age frequently but not always lags behind the chronological age. In No. 818, where death was due to intussusception, skeletal age and chronological age are identical. This is in accordance with the well-known clinical observation that intussusception usually occurs in healthy well-developed boy babies. Other examples of well-grown, well-matured children can be picked out from Table 9. They form marked contrasts with the children stunted in growth and retarded in maturity.

The table is arranged according to skeletal age (bodily maturity) and, where one skeletal age has been assigned to several children, the eyeballs of these children have been set down in order of magnitude. By use of the formulae already given, all eyeball weights have been reduced to male white equivalents. Thus sex and race have been, so far as possible, discounted. By using skeletal age as a basis the influence of progressive bodily maturity has been cancelled out. This leaves bodily dimension or size as the variable to be checked against eyeball weight.

A little study of Table 9 indicates pretty clearly that size (bodily stature) in itself is not so well correlated with eyeball weight. It is

TABLE 9

Growth of eyeball

Eyeballs arranged according to skeletal age (epiphysial rating during fetal life, maturation rating after birth). This device brings the eyeball weight, corrected to its male white equivalent, into orderly progress and shows the lack of relationship to bodily maturity, bodily size or chronological age.

SKEL. AGE	EP. AGE*	CHRON. AGE	STATURE AGE	SEX COLOR	CAD. NO.	AV. WT. EYEBALL	LEFT EYE	RIGHT EYE	CORR. EYE WT.
6th lunar mo.		12 days	7.75 lunar mos.	MW	2023	1.55	1.60	1.50	1.55
6th lunar mo.		9 days	8 lunar mos.	FW	2149	1.20	1.00	1.40	1.29
7th lunar mo.		1 day	8 lunar mos.	MN	1715	2.20	2.20	2.20	2.03
7th lunar mo.		16 days	8 lunar mos.	MN	1354	1.68	1.66	1.70	1.55
7th lunar mo.		1 mo. 7 days	9 lunar mos.	MW	1836	1.60	1.50	1.70	1.60
8th lunar mo.		26 days	8.75 lunar mos.	MW	1405	2.46	2.44	2.48	2.46
8th lunar mo.		26 days	9 lunar mos.	FW	1406	2.34	2.36	2.32	2.51
8th lunar mo.		10 days	9.5 lunar mos.	MN	2134	1.80	1.70	1.90	1.66
8th lunar mo.		21 days	9.5 lunar mos.	MN	1689	2.75	2.75	2.75	2.53
8th lunar mo.		1 mo. 14 days	9.5 lunar mos.	FW	2119	2.47	2.45	2.50	2.65
8th lunar mo.		7 days	2 weeks	FW	1620	2.52	2.52	2.52	2.70
9th lunar mo.		21 days	8.5 lunar mos.	FN	1883	2.00	2.00	2.00	1.97
9th lunar mo.		16 days	10 lunar mos.	MN	2183	2.65	3.00	2.30P	2.44
9th lunar mo.		1 mo. 7 days	10 lunar mos.	MN	1599	3.72	3.70	3.75	3.43
						Cerebral Haem. since birth			
9th lunar mo.		3 mos. 8 days	1 week	MW	1867	2.72	2.75	2.70	2.72
9th lunar mo.		2 months	2 weeks	MW	1968	3.12	3.25	3.00	3.12H

TABLE 9 (*Continued*)

SKEL. AGE	EP. AGE*	CHRON. AGE	STATURE AGE	SEX COLOR	CAD. NO.	AV. WT. EYEBALL	LEFT EYE	RIGHT EYE	CORR. EYE WT.
9th lunar mo.		2 mos. 13 days	3 weeks	FW	1450	2.76	2.80	2.72	2.96
10th lunar mo.		3 mos. 5 days	10 lunar mos.	FN	2238	2.10	2.00	2.20	2.07
10th lunar mo.		Birth	1.5 months	MW	1971	3.22	3.30	3.15	3.22H
10th lunar mo.		14 days	1.5 months	MW	1461	2.76	2.80	2.72	2.76
Birth	3 weeks	24 days	2 months	MW	1621	3.50	3.50	3.50	3.50
Birth	1 month	1 mo. 12 days	9.5 lunar mos.	MW	1457	2.28	2.26	2.30	2.28
Birth	1 month	1 mo. 10 days	5 weeks	FW	1462	3.22	3.20	3.24	3.46
ca. 1.5 wks.	2 months	11 days	9.5 lunar mos.	MN	2124	2.70	2.80	2.60	2.49
ca. 1 mo.	2 months	3 mos. 12 days	5 weeks	FN	1847	3.25	3.20	3.30	3.20
ca. 1 mo.	2 months	3 months	1.5 months	FN	1432	3.68	3.68	3.68	3.63
ca. 1 mo.	2 months	3 mos. 12 days	1.5 months	FN	1701	3.60	3.60	3.60	3.55
ca. 1 mo.	2 months	1 mo. 22 days	2 months	FN	1868	3.10	3.10	3.10	3.05
ca. 1 mo.	2 months	5 mos. 10 days	2.5 months	FW	1861	3.00	3.00	3.00	3.22
ca. 1 mo.	2 months	3 months	3 months	FW	1826	3.25	3.30	3.20	3.49
ca. 1 mo.	3 months	1 mo. 1 day	2 weeks	FW	1916	2.79	2.78	2.80	2.99
ca. 1 mo.	3 months	2 months	2 months	MN	2233	2.85	3.00	2.70	2.63A
ca. 1.5 mos.	2 months	1 mo. 17 days	1.5 months	FN	1963	3.40	3.40	3.40	3.35
2 months	2 months	4 mos. 7 days	3 months	MN	1391	3.22	3.20	3.25	2.97
2 months	2 months	4 mos. 2 days	3.5 months	FN	1700	5.00	4.98	5.02	4.93
2 months	3 months	2 mos. 10 days	2.5 months	FN	1879	3.95	4.00	3.90	3.89
2.5 months	2 months	6 months	2 months	MN	2010	4.40	4.35	4.45	4.06
2.5 months	3 months	6 months	4 months	MW	2200	3.80	4.00	3.60	3.80

TABLE 9 (*Continued*)

SKEL. AGE	EP. AGE*	CHRON. AGE	STATURE AGE	SEX COLOR	CAD. NO.	AV. WT. EYEBALL	LEFT EYE	RIGHT EYE	CORR. EYE WT.
3.5 months	3 months	8 months	11 months	MW	1936	5.20	5.20	5.20	5.20H
4 months	3 months	4 months	5 months	MW	818	4.00	4.10	3.90	4.00
4 months	3 months	1 yr. 5 mos.	6 months	MN	1379	4.39	4.42	4.37	4.05
4 months	6 months	9 mos. 13 days	4 months	FW	1801	3.42	3.35	3.50	3.67
5 months	3 months	5 mos. 17 days	5 months	FN	1626	4.05	4.05	4.05	3.99
5 months	10 months	6 mos. 18 days	7 months	FW	1619	4.49	4.47	4.52	4.82
5.5 months	3 months	10 mos. 10 days	5 months	FN	1453	4.82	4.82	4.82	4.75
6 months	3 months	7 months	6.5 months	MN	1148	4.56	4.58	4.52	4.20
6 months	10 months	10 months	6 months	FN	2234	4.00	4.00	....	3.94A
6 months	7 months	9 mos. 24 days	7.5 months	FW	2081	4.65	4.50	4.80	4.99
9 months	3 months	11 months	8 months	MN	1187	5.06	5.06	5.06	4.67
11 months	3 months	1 yr. 2 mos.	10 months	MN	1894	5.75	5.80	5.70	5.30
12 months	11 months	1 yr. 3 mos.	12 months	MN	2075	6.35	6.50	6.20	5.86
1 yr. 4 mos.	10 months	2 yrs. 0 mos.	1 yr. 6.5 mos.	FN	1435	4.30	4.30	4.30	4.24
1 yr. 7 mos.	1 yr. 6 mos.	1 yr. 1 mo.	1 yr. 9 mos.	MN	1385	6.32	6.32	6.32	5.83
2 yrs. 4 mos.	1 yr. 10 mos.	3 yrs. 1 mo.	2 yrs. 8.5 mos.	FN	1509	5.48	5.44	5.50	5.40
2 yrs. 7 mos.	1 yr. 11 mos.	3 yrs. 1 mo.	2 yrs. 0 mos.	MN	1557	7.22	7.22	7.22	6.66
4 yrs. 8 mos.	1 yr. 8 mos.	3 yrs. 5 mos.	4 yrs. 5.5 mos.	MN	2240	6.90	6.50	7.30	6.36
5 yrs. 6 mos.		5 yrs. 6 mos.	4 yrs. 7 mos.	FN	1874	6.22	6.25	6.20	6.13
8 yrs. 9 mos.		7 years	7 yrs. 4.5 mos.	MN	1249	7.58	7.55	7.61	6.99
8 yrs. 11 mos.		13 years	7 yrs. 5.5 mos.	FN	2118	6.50	6.70	6.30	6.41
8 yrs. 11 mos.		8 yrs. 9 days	8 yrs. 2.5 mos.	MN	1834	7.60	7.70	7.50	7.01

TABLE 9 (*Continued*)

SKEL. AGE	EP. AGE*	CHRON. AGE	STATURE AGE	SEX COLOR	CAD. NO.	AV. WT. EYEBALL	LEFT EYE	RIGHT EYE	CORR. EYE WT.
9 yrs. 6 mos.		7 years	3 yrs. 7 mos.	FW	2036	6.02	5.75	6.30	6.47
11 yrs. 7 mos.		10 years	9 yrs. 6 mos.	MN	1441	6.44	6.40	6.48	5.94
11 yrs. 11 mos.		12 years	9 yrs. 10 mos.	FW	1772	6.96	7.05	6.87	7.48
14 yrs. 11 mos.		17 years	13 yrs. 0 mos.	MN	1589	6.23	6.80	5.67b	5.75
15 yrs. 3 mos.		17 years	12 yrs. 9.5 mos.	MN	1711	7.80	7.80	7.80	7.19
16 yrs. 6 mos.		18 years	over 18 yrs.	FN	1590	8.02	8.04	8.00	7.91
18 yrs. 1 mo.		18 years	over 19 yrs.	MN	1974	8.20	8.30	8.10	7.56
18 yrs. 2 mos.		21 years	17 yrs. 1 mo.	MN	1782	7.92	7.95	7.92	7.21
18 yrs. 5 mos.		21 years	over 19 yrs.	MN	1729	7.65	7.20	8.10	7.06

*—Epiphysial age and skeletal age are practically identical until birth.
H—hydrocephaly.
P—This eyeball was unusually distorted.
A—Approximate value. The right eye was slit on removal.
b—Right eye reported as unusually small.

evident that the influence which retards stature progress, like that retarding bodily maturity, if it occurs during the first year when the eyeball is growing rapidly, has a profound effect on eyeball weight in later childhood and, by inference from Jackson's work (1936), in adulthood. Without much more evidence no unqualified assertion can be made, but it does look as if the retarding influences which repressed growth in dimension were even more potent than the normal growth and maturation factors in controlling eyeball size. If so, this also would be consonant with Jackson's findings on rats.

When the end of Table 9 is reached the eyeball weight is that of the adult as given in Table 8. There are, however, very considerable differences in adult weight as is evident from the standard deviation. These differences are more pronounced in Cleveland Negroes than in Cleveland whites. If we are right in our inference that influences retarding physical growth and maturity have a profound effect on eyeball growth and on ultimate eyeball weight the explanation of this large variability exaggerated in Negroes is clear. Table 9 shows that adult size of eyeball is not attained in early childhood, which indeed terminates what Keith long ago called the functional concomitant of growth in contradistinction to the corporeal concomitant of later childhood. There is evidently a gradual increase in size of eyeball up through adolescence.

Cadavers 1936, 1968, 1971 were cases of hydrocephaly in which, it will be noted, eyeballs are unusually large. These three together with Cadaver 1599 (cerebral hemorrhage since birth) are omitted from the graph.

Growth progress in eyeball weight can be readily appreciated from Fig. 1 which has been constructed on principles governing Table 9. The average eyeball weight of each child has been converted into the corresponding male white value by multiplying the weight by the appropriate factor defined earlier in this paper. The results are plotted as ordinates against chronological age (dots) and against skeletal age of bodily maturity (crosses). The limits of each range are indicated by the arbitrary defining lines. The relative compactness of the area curve shows that the results have been placed on a satisfactory common plane and the steepness of both area curves near their origin indicates the rapidity with which development to childhood weight takes place. Inasmuch as velocity of growth is sought, the steeper or skeletal age curve is the more significant. That means that growth of eyeball is more directly related to general physiological development than to chrono-

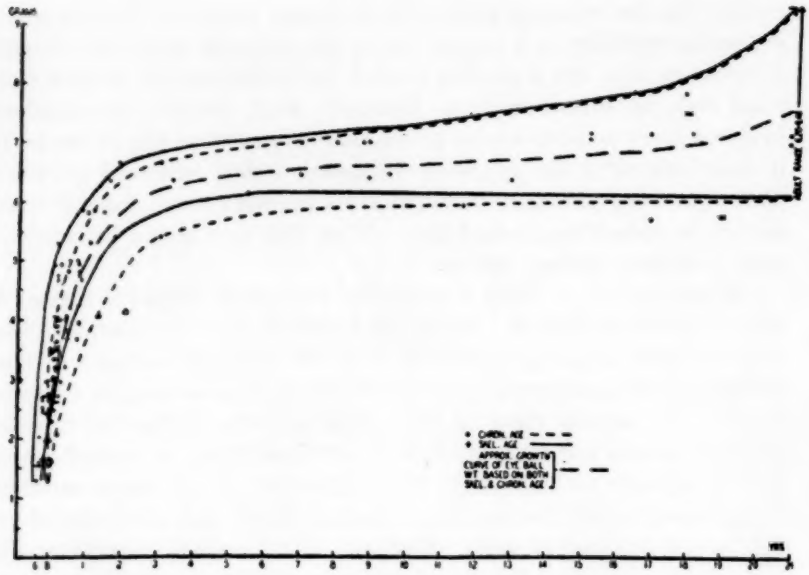

FIG. 1. GROWTH OF EYEBALL BY CHRONOLOGICAL AGE (DOTS). BASED ON WEIGHTS OF 65 PAIRS OF EYEBALLS BETWEEN BIRTH AND 21 YEARS. ALL ARE CORRECTED TO MALE WHITE VALUES

Note considerable growth during first year continued with diminished velocity to third birthday followed by a secondary growth possibly focussed near adolescence when adult size is reached. The considerable range recorded in adult eyeball weight suggests susceptibility of the eyeball to adverse influences during its period of rapid growth. This is discussed in the text.

GROWTH OF EYEBALL BY PROGRESS IN SKELETAL AGE (BODILY MATURITY) (CROSSES)

Note that this curve is steeper during infancy indicating that eyeball growth is more closely related to general physiological development than to chronological age.

logical age. As one might expect, these data drawn from the eyeballs of dead children are but poorly expressed in terms of chronological age. A dead child, unless killed by traumatic accident, burns or scalds, intussusception or possibly pneumonia, is apt to be a child defective in developmental growth as a result of the retarding influence of disease.

The chart shows astonishing velocity of growth during the first year. By the third birthday the approximate childhood stage is reached. The chart thereafter shows no growth till about puberty when a second spurt

of growth brings the eyeball to adult level. This may be a genuine spurt of growth, but until more evidence accumulates, one would be wiser to express it as a gradual increase during childhood.

Comparison with growth of the eyeball in the rat, as an example of mammalia, may be made by reference to articles by Donaldson and King (1937), Hatai (1913) and Jackson (1913).

Now it may be argued that we should not apply to children's eyeballs a correction factor based on relative weights of adult eyeballs. We do not hold any brief for this method except its vividness in presentation and its acceptability on quite general lines as indicating the trend of eyeball growth in a small and heterogeneous sample. Whatever argument may be adduced against the method the basic fact is demonstrated, namely that the eyeball reaches approximate childhood level by the third birthday and attains adult dimensions by a second spurt of growth during or before adolescence.

The data gathered in Table 9 and the chart confirm the general conclusions shown in Curventafeln I and II in the article by Weiss, and in Figures 18 and 19 in the article by Scammon and Armstrong. They modify the conclusions drawn by Tamaru (1929) who thought that growth during the first year was negligible. It is true, of course, that by far the greatest growth does occur in fetal life but that there is considerable growth during the first year and then greatly diminished velocity of increase until three years (and later childhood) is evident from the articles by Weiss and by Scammon and Armstrong as well as from our own Table 9 and chart.

The clinical inference to be drawn from these findings is that growth of the eyeball is rapid only in infancy. The velocity of growth is greatly reduced in the preschool period. Defects of vision have completed their adverse effect on eyeball growth in quite early childhood. Correction of the defect of vision must therefore be established far earlier than is at present the general custom if this correction is to serve as a restorative influence rather than a crutch for crippled or disabled organs.

SUMMARY

1. This study of 150 pairs of adult eyeballs and a growth series of 69 pairs gives data upon growth in weight determined through distension of eyeball by water to restore approximate living intra-ocular tension.

2. Experiments on technique and comparison with weights obtained by earlier investigators give confidence in the method of study.

3. In the 150 pairs of adult eyeballs, right is larger in 56 and left in 39. In the remaining 55 pairs, right and left are of equal weight.

4. Eyeball growth in general follows brain growth but final adult eyeball weight shows no correlation with cranial capacity (the indicator of brain size). Indeed the eyeball seems quite susceptible to those influences which retard maturity and growth in dimension, perhaps particularly the latter.

5. The Negro eyeball is larger than that of the white. Female eyeballs, though absolutely smaller, are relatively larger than male eyeballs.

6. The difference in weight between right and left eyeballs is usually due to error in determination. A real difference in size rarely occurs.

7. No real change in eyeball size after adult weight is reached can be demonstrated by existing methods.

8. Approximate childhood weight is reached by the age of three years. The conclusion of earlier observers that most of the growth takes place in fetal life is confirmed but our data point to considerable growth during first year continued with diminished velocity to the third birthday and followed by a secondary growth possibly focussed near adolescence.

9. The eyeball growth pattern is dependent both on developmental progress and upon chronological age.

10. The eyeballs of children dying in early life are not trustworthy as indicators of the growth of the eyeball in healthy living children of equal chronological age.

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THE QUESTION OF MIRROR IMAGING IN HUMAN ONE-EGG TWINS

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WHEN the writer over thirty years ago first became interested in twins, the idea was already current that human identical twins are more or less mirror-image duplicates. Who was the first writer to note that quite commonly one member of a pair of such twins is right-handed and the other member left-handed and to cite this as an example of mirror imaging, we have not been able to ascertain. Certainly the idea is an old one and had enjoyed rather general acceptance.

One of the pioneers of the general concept of mirror imaging in one-egg twins was the late H. H. Wilder whose notable researches in the field of human twinning date from the beginning of the present century. In one of his earlier papers ('04) Wilder pointed out that in a number of "duplicate" twins studied by him the palm patterns were definitely mirror-imaged, the right palm of one twin being like the left of the other, and vice versa.

Since Wilder's time many other students of human twins have called attention to the occurrence of mirror imaging in handedness, hair whorl, dentition, ear form, palm patterns, and other more or less asymmetrical characters. The theory commonly advanced to account for these types of mirror imaging in one-egg twins is that a pair of such twins is derived from the prospective halves of an originally single embryo that had already begun to differentiate its right and left sides in mirror-image fashion. If this theory of the origin of one-egg twins were accepted, two difficulties immediately present themselves: first, that the majority of pairs of one-egg twins show no mirror imaging, and second, that mirror imaging involves some asymmetrical structures and not others.

The present writer ('28), as the result of a study of 50 pairs of one-egg twins, noted that mirror imaging in handedness, hair whorl, palm

patterns, etc., seemed to be rare or absent in the most similar pairs and rather common among the less similar pairs of one-egg twins. This situation gave rise to the theory that twinning in man occurs at various stages of development, ranging from early cleavage to late gastrulation. If twins result from a splitting of an early cleavage stage or through the twinning of an inner-cell-mass, the resulting twins would be expected to show no mirror imaging because right-left differentiation had not begun when the twinning division occurred. Twins, however, resulting from later division after right-left differentiation had made some progress, would be expected to show progressively more extensive mirror imaging the later the twinning division occurred, and along with mirror imaging would go increasing differences. Siamese twins were regarded as the product of a division that came too late to be able to complete itself. In such twins it is well known that mirror imaging is the rule rather than the exception. In 1931 the writer called attention to the fact that Siamese twins not only exhibit extensive mirror imaging, but that the two members of a pair differ more than do separate one-egg twin pairs with respect to such characters as height, head shape, facial features, palm patterns, and other characters. Such differences were regarded as secondary consequences of a late separation of two body halves that, before twinning occurred, had developed different potentialities. It was also suggested that mirror imaging was the primary factor in producing intra-pair differences and that differences of other sorts were secondary consequences of mirror imaging.

Various aspects of these theories have met with critical opposition, but the one item of the theory that seems to have given rise to most opposition is the view that mirror imaging in separate one-egg twins is the result of the separation of the two prospective sides of an embryo that had begun to develop opposite asymmetries. It is this particular sub-theory that we shall ourselves examine critically and modify in the light of new facts that have come to our attention in recent years, facts that throw new light on the nature and causes of asymmetry reversal and mirror imaging both in twins and in single individuals.

Perhaps the most important contribution to a better understanding of mirror imaging is a study by T. Komai ('38), the leading Japanese student of twinning, on "The problem of situs inversus viscerum, as studied in single and duplicate salmon embryos." Situs inversus viscerum is the most extensive expression of asymmetry reversal of which a vertebrate is capable. Such reversal may be perfect or imperfect. In the following review of Komai's results we shall, following his example,

abbreviate the long and awkward Latin phrases *situs inversus viscerum perfectus* to s. i. v. p. and *situs inversus viscerum imperfectus* to s. i. v. i. Since this study of s. i. v. in salmon embryos is basic for all that follows, we shall try to present the gist of it as briefly as possible.

TWINNING AND S. I. V. IN SALMON EMBRYOS

Komai's paper is packed with significant data, but his discussion of the broader implications of the new findings is rather meager. In the collection of his data Komai employed well-trained Ainu women, who where instructed to sort over many thousands of embryos in the fish hatcheries and select all that were in any way atypical, including single anomalies and all types of complete and partial twinning. The total material collected and studied, all of which had to be carefully dissected for the study of s. i. v., consisted of 200 normal single embryos, 430 more or less abnormal or deformed single embryos (not twins), 502 sets of completely separate twins (united only by the common yolk sac), and 287 sets of partially duplicate twins (double monsters) most of which consisted of various grades of anterior duplicity. This material is so extensive that statistical comparisons can be made and definite conclusions drawn therefrom.

The first fact to emphasize about this study is that s. i. v. was found in all types of embryos studied. Among the 200 normal single embroys, 13 (6.5 per cent) showed incomplete s. i. v.; among the 430 deformed single embryos, 33 (7.6 per cent) showed s. i. v. p. and 55 (12.8 per cent) showed s. i. v. i.; among the 502 sets of separate twins, 88 (17.5 per cent) showed s. i. v. It is clear from these facts that s. i. v. is not necessarily due to twinning, for it occurs in single embryos, especially among those that are deformed. It is equally clear, however, that s. i. v. is much commoner among twins than among single embryos, and is especially prevalent among double monsters. This suggests that twinning and s. i. v. are at least causally related.

A study of the 502 sets of separate twins revealed that 219 sets consisted of equal-sized and 283 sets of unequal-sized twins. Among the equal-sized sets one or both twins were deformed in 18.7 per cent of the cases, while among the unequal-sized sets 74.2 per cent showed more or less marked deformity. *There is thus a strong correlation between size inequality and deformity.* It was also found that s. i. v. was three times as frequent in the deformed as in the undeformed member of these pairs, thus showing *a strong correlation between deformity and*

s. i. v. The same close correlation between underdevelopment, deformity and s. i. v. was found to exist in the 287 sets of partially duplicate twins. Another striking set of facts came to light in the study of these conjoined twins, namely, that right and left components were very unequally affected. It was found that the right member was smaller in 29 sets and the left member smaller in 12 sets; that the right member was deformed in 47 sets and the left member deformed in 33 sets; and that the right member showed s. i. v. in 66 sets and the left member in 31 sets. *There is thus a consistent tendency for the right member of conjoined twins to be underdeveloped, deformed and to show s. i. v.*

Komai deduces from the above facts three important conclusions: first, that s. i. v. is "a result of a disturbance of the normal developmental process, much as the small size and deformation themselves;" second, that incomplete twinning such as is found in the partially duplicate twins "makes the appearance of s. i. v. easier;" and third, that the right-sided predominance of smaller size, deformity and s. i. v. is a consequence of "a kind of physiological gradient passing from left to right of a fish embryo. The derangement of this gradient brings about the situs inversus viscerum."

Komai makes no suggestion as to the causes responsible for all these disturbances of normal development, but it seems rather obvious that not merely are underdevelopment, deformity and s. i. v. caused by the subnormal environmental conditions connected with the artificial rearing of embryos in fish hatcheries, but that twinning is also caused by the same conditions.

That complete and partial reversals of asymmetry can readily be induced by growth-depressing agents was clearly shown by the writer ('25) in connection with echinoderm embryos.

ASYMMETRY REVERSAL IN ECHINODERMS

As is well known, echinoderms are secondarily radially symmetrical as adults, but the early larvae are bilaterally symmetrical. In later larvae, however, development becomes profoundly asymmetrical through the appearance of a hydrocoele and water pore on the left side only. Around the hydrocoele the adult radial symmetry is developed. Rare specimens of both starfish and sea urchin larvae have been found in nature in which the asymmetric growth is reversed, the hydrocoele and water pore appearing on the right side. This is equivalent to a sort of

exaggerated s. i. v. The writer ('25), as well as several other experimenters, has found that larvae of echinoderms reared under somewhat subnormal conditions in the laboratory undergo this kind of s. i. v. in considerable numbers, and if the very young larvae are chilled down to nearly freezing temperature for an hour or so, the percentage of cases of s. i. v. is markedly increased. In addition to typical cases of s. i. v. not a few larvae develop the hydrocoele and water pore on both right and left sides, usually resulting in a twin condition with one side more normal than the other.

The physiological explanation offered for these anomalous conditions was as follows: that adverse laboratory conditions, and especially low temperature, tend to level the physiological imbalance between the left and right sides, the left side being in many cases more inhibited. When normal conditions are resumed the less inhibited right side would recover first and thus become the superior side. The right side would then dominate the left and prevent it from assuming the leading rôle in further development. It is also conceivable and probable that in some cases the two sides might recover simultaneously after normal conditions are restored and that both sides would then be so nearly on a par physiologically that each would exercise a certain degree of dominance over the other, thus producing partial s. i. v. on both sides. This explanation would apply to a number of cases of so-called twinned larvae in which somewhat reduced hydrocoeles and hydropores persist on both sides up to an advanced brachiolaria stage. In the great majority of individuals in all experiments, however, the left side regains its dominance and this prevents asymmetry reversal.

Whether we speak of the difference between the left and right sides of the body as due to a left-right physiological gradient or as due to a physiological dominance of the left over the right side, makes but little difference as to the interpretation of cases of asymmetry reversal (s. i. v.) in echinoderm larvae. In either case the condition seems to be the result of a disturbance of normal physiological relations due to lowering the metabolic rate to such an extent that, at least temporarily, the left-right gradient is abolished or the left superiority is diminished. Subsequently recovery gives the two sides equal chances to become superior. As a rule the original superior side regains its dominance, but in many larvae the right, or normally inferior side, gains the ascendancy and s. i. v. results.

It need hardly be pointed out that the physiological explanation of s. i. v. adduced from echinoderm experiments can be directly applied to

the data furnished by Komai on salmon embryos. All we need to assume is that the conditions in fish hatcheries are somewhat subnormal, and tend to act as physiological depressants. One-egg twinning is one of the commonest consequences, but s. i. v., either in twins or in non-twinned embryos, may be regarded as another common consequence of the subnormal environment. It is highly significant, moreover, that twinning and s. i. v. are definitely, but not always, correlated, and that s. i. v. is more often than not associated with under-sized and deformed components of twin pairs. It must not be forgotten, however, that s. i. v. occurs independently of twinning.

That both twinning and mirror imaging in general are consequences of growth depressing agents is, furthermore, strongly indicated by some rather recent studies by Keller and Niedoba ('37) on conjoined twins in cattle. These observations bring the problem somewhat closer to the human level.

DOUBLE MONSTERS IN CATTLE

In 1937 Keller and Niedoba published a report on the conditions found in 20 advanced double-monster calf fetuses, and accompanied their description with a general discussion of the bearing of their results on general theories of twinning. Almost all cases consisted of anterior duplicities of varying extent. In other words, most of the fetuses were more or less completely two-headed. Keller and Niedoba were particularly impressed by two conditions in these fetuses: the frequency of abnormalities and prevalence of mirror imaging. The commonest abnormality was cleft palate, a stock example of a defect due to developmental arrest. Most of the double-monster fetuses were also abnormal in other ways. Several showed spina bifida, crytorchid testes, hyposadia, reduced brain, hair lip, ankylosed neck vertebrae, bifurcated tongue, etc. In six of the twenty cases mirror imaging with respect to hair whorls and color markings was obvious, and in several other cases mirror imaging was evident to a lesser degree. No data were given as to the incidence of s. i. v.

This association of partial twinning, abnormalities and mirror imaging is similar to that found in partially duplicate salmon embryos and the conclusion seems unavoidable that these conditions in mammalian twins are also the result of growth-depressing agents.

Keller and Niedoba agree with the writer that partners of conjoined twins differ far more than do separate one-egg twins, but their

conclusions from this fact differ from ours. They were so impressed with the extent of abnormality in these calf double monsters that they are inclined to regard partial twinning and the abnormalities associated with it as a result of some damage to the embryo, which may be due either to genetic or environmental factors, but they regard it still very doubtful that separate one-egg twinning is caused by similar factors. They claim that separate one-egg twins are as a rule fundamentally normal beings, while double monsters are in their nature actually *Missbildungen*, i.e., deformities or abnormalities.

In reply to this view, it may be said that the cases of fundamentally normal separate one-egg human twins that are both born alive and grow up to adult life constitute a highly selected group, for most of the abnormal fetuses fail to live long enough to be born. Also it seems only fair to refer back to the really adequate data furnished by Komai's salmon twins. There it was shown that separate one-egg twins exhibit the same deformities, including s. i. v., as do the conjoined twins, though with a lower frequency. From this it may be inferred that separate one-egg twins and double monsters in calves and other mammals are due to the same kinds of depressing factors. Now we are ready to examine the human situation in more detail.

MIRROR IMAGING IN MAN

S. i. v. in single human individuals

S. i. v. is defined by Cleveland ('26) as "that condition in which normal arrangement of the viscera is reversed to form a mirror picture of the usual position. Such transposition (in single individuals) is usually total but may in rare instances involve either the thoracic viscera or the abdominal viscera alone." It seems to us highly probable that in human subjects only extensive reversals of asymmetry have been noticed and that minor degrees of it have been overlooked or else regarded as anomalies unrelated to s. i. v.

Cleveland has attempted to collect all of the described cases of human s. i. v. up to the date of his paper. He published a list of references to over 100 reports, describing about 125 cases of complete or partial s. i. v. in singly-born human subjects. The majority of these cases had come to light in course of dissection in anatomical laboratories, though not a few had been discovered through the use of the stethoscope and confirmed by X-ray photography.

The frequency of s. i. v. in single individuals is not known, but must be very low. In the anatomic laboratory of Columbia University College of Physicians and Surgeons during forty years about 10,000 subjects had been dissected and only one case of s. i. v. has been found. In the Mayo Clinic only ten cases of s. i. v. were found among 347,000 patients registered over a twelve-year period. Le Wald ('25) reports as the result of X-ray examination of some 40,000 individuals that twenty-nine cases of s. i. v. were found. Thus in about 400,000 individuals examined by physicians only thirty-nine cases of s. i. v. were discovered. This is about one case in 10,000.

In the literature on s. i. v. frequent references are made to the existence in these reversed individuals of various apparently unrelated abnormalities and anomalies. Ludwig ('32), in his treatise on the right-left problem in the animal kingdom and in man, was particularly struck by the association of s. i. v. with other anomalies. He states that human cases of s. i. v. are commonly associated with other structural abnormalities, such as hair-lip, hexadactylism, atresia of the digestive tract and malformation of the heart. Most of these conditions are generally regarded as developmental arrests of one sort or another and as due to a lowering of developmental rate at critical moments during early development.

S. i. v. in separate one-egg twins

So far as we have been able to discover only five cases of separate one-egg human twins have been reported as exhibiting s. i. v. in one of the twin partners. These conditions were discovered mainly during autopsies of still-born twins or those that died in early infancy. No systematic examination of large numbers of one-egg twins has ever been made with the object of searching for possible cases of perfect or imperfect s. i. v. Were such a search made by means of X-ray photographs we suspect that not a few cases of s. i. v. would come to light. In this connection it is worthy of note that both Spaeth (1860) and Schatz (1882-1910) have noted in separate one-egg twins not a few cases of still-born separate one-egg twins in which minor deviations from the normal asymmetry of the blood vessels are present. These are interpreted as cases of partial s. i. v. Schatz has called attention to the fact that such partial reversals of the blood vessels are of common occurrence in twins that are abnormal in other ways.

S. i. v. in conjoined human twins

This is hardly the place to attempt a classification or description of the numerous and diverse types of human conjoined twins and double monsters. One may find such classifications in almost any treatise on human teratology. A few statements, however, seem necessary and pertinent to the present discussion. There is now no question as to the one-egg origin of conjoined twins. The old idea that they are the result of the fusion of two originally separate embryos has been entirely abandoned. There is no doubt, however, that some types of "grown-together" twins have been the result of a secondary fusion of two separate embryonic axes developed on a single blastoderm. Where one finds the twin components placed symmetrically side by side, however, it is far more probable that they are the product of a partial fission of a single embryonic axis. Moreover, those that are joined side by side are the only kinds of twins in which the two components can be distinguished as right or left members of the pair—a circumstance that has considerable bearing on the question of s. i. v., as has already been brought out in this discussion.

It is difficult to obtain detailed information as to the incidence of s. i. v. in human conjoined twins, but it is clear from statements made by authors who have covered the scattered literature on the subject, that s. i. v. in one of the components of such pairs is the general rule while its absence is the exception. Just as was the case in partially duplicate salmon twins, the right component is usually the one that is reversed in the asymmetry of the viscera. From the available literature one finds also that the two components of conjoined human twins are as a rule unequal in size. Differences between the two components range from relatively slight inequalities in height, head dimensions, muscular development, facial features, etc., such as are found in most pairs of viable conjoined twins, to extreme cases of autosite and parasite. In the most extreme cases of the latter type the parasite twin may be reduced to a formless cyst attached to the tongue or enclosed in the body cavity of the autosite twin.

Even within that range of conjoined twins in which both components seem normal and healthy it is usually evident that one twin is smaller and somewhat subnormal. As an example of this we may cite the case of the Bohemian sisters, in which the right-hand component was defective with respect to uterus, which was infantile, while that of her twin sister was so normal that she had produced a normal child.

In the case of the Gibb sisters the right component had a decidedly subnormal face and was less normal in other ways.

From the literature describing the anatomy of still-born human conjoined twins we gather that as a rule the less well-developed component more commonly exhibits s. i. v. than does the more normal component, but there are a few cases in which the opposite is true. Also it is fairly clear from discussions of large numbers of cases that, in those twins in which the two components can be identified as right and left, the right component far more commonly than the left shows perfect or imperfect s. i. v. Even here, however, the situation is occasionally reversed, the left component exhibiting s. i. v. My search has not revealed a single case in which s. i. v. is expressed in both components.

It is clear from the above discussion that in human beings s. i. v. is more frequent in conjoined than in separate one-egg twins, that s. i. v. is more prevalent in the less-developed component, in the component exhibiting other abnormalities, and is much commoner in right than in left components of conjoined twin pairs. It need hardly be pointed out that, so far as the facts are known, the s. i. v. situation in human twins closely parallels that in salmon twins. It seems as necessary in one case as in the other to explain s. i. v. as one of the consequences of growth depressing agents.

CORRELATION BETWEEN S. I. V. AND OTHER TYPES OF ASYMMETRY REVERSAL

When one finds instances of normal situs in one twin of a pair and s. i. v. in the other, there can be no doubt but that one twin has undergone an epigenetic reversal of the genetically prospective asymmetry of the single zygote from which the twins were derived. Anyone will freely admit that such a situation implies genuine mirror imaging in separate one-egg twins.

It is a far cry from the deep-seated and far-reaching type of asymmetry reversal seen in s. i. v. to the rather minor and superficial types of mirror imaging such as those in handedness, hair whorl, palm patterns, ear shapes, dental irregularities, etc. With respect to these characters, there are wide divergences of opinion as to whether they ever undergo genuine epigenetic reversal in one member of separate one-egg twin pairs. It is true that in about a fifth of such pairs one is a right and the other a left hander, in about the same proportion of pairs one has clockwise and the other counterclockwise hair whorl, and in a somewhat smaller

proportion of pairs the right palm of one twin resembles the left palm of the other much more than his own or his partner's right. Some, including the present writer, regard such conditions as true mirror imaging. Others, especially v. Verschuer, have been inclined to regard these conditions as mere "paratypical" mirrorings caused by more or less accidental differences in prenatal or postnatal environment.

Verschuer and others have pointed out that mirroring with respect to handedness and hair whorl is almost as common in two-egg as in one-egg twins and argue from this that mirror imaging in both must have the same origin. It seems to us, however, that the two situations are utterly different. A pair of one-egg twins is identical genetically while a pair of two-egg twins is unlike genetically. When mirroring with respect to an asymmetrical character occurs in a one-egg pair surely one of the two must express the prospective asymmetry of the zygote from which they have been derived and the other twin must have undergone a reversal of this prospective asymmetry. The actuality of mirror imaging is not in the least diminished by the fact that there are some two-egg twin pairs in which the two individuals show opposite asymmetries. The only way in which the genuineness of mirror imaging in one-egg twins can be invalidated is by denying that asymmetrical characters under discussion have a genetic basis. Verschuer has gone so far as to doubt that handedness has a genetic basis, but there is far too much evidence to the contrary to make his view acceptable. Once admit that handedness is hereditary, there is no escape from concluding that a pair of one-egg twins, one a right and the other a left hander, exhibits real mirror imaging in handedness. The same arguments hold for mirroring in hair whorl, palm patterns, ear form, dentition, and other characters of a more or less superficial sort.

The gap between s. i. v. and these minor expressions of mirror imaging has been partially filled by Keeler ('29), who compared 10 sets of human double monsters with 14 pairs of separate one-egg twins with respect to the comparative frequencies of mirrorings in palm and sole patterns, hair whorls, dental irregularities and ear shapes. He found that in the laterally conjoined twins 77 per cent of these characters showed mirror imaging, as compared with only 22 per cent for the separate twins. In other words, the superficial types of asymmetrical characters showed just about the same relative frequency in the two kinds of twins as does s. i. v. Hence there is a close correlation between the incidence of s. i. v. and that of these other types of asymmetry re-

versal, a fact that strongly supports the view that the latter should be regarded as instances of true mirror imaging.

It seems to us that there has been a considerable amount of over-anxiety to discredit the concept of mirror imaging in one-egg twins. The motive back of the attack is not very clear, but reading between the lines, we surmise that twin specialists in general would be glad to get rid of the asymmetry reversal mechanism as a difference-producing factor, distinct from genetic and environmental factors. Now that we are willing to admit that asymmetry reversal and its corollary mirror imaging are in all probability due to environmental disturbances, it may not seem so important as formerly to explain them away or to deny their reality. Mirror imaging is real and obvious in one-egg twins. The only difference of opinion about it should be not as to its reality but as to its causes.

WHAT CAUSES MIRROR IMAGING?

The facts brought out by Keeler when he found that conjoined twins, which exhibit much more extensive s. i. v. than do separate one-egg twins, also exhibit much more extensive mirror imaging in such external characters as palm patterns, ear shapes, hair whorl, etc., strongly suggest that s. i. v. and minor degrees of asymmetry reversal are different expressions of the same phenomenon and are due to the same causes, namely, growth-depressing factors.

The fact that in conjoined human twins the right component far more frequently than the left exhibits a reversal of normal asymmetry proves that mirror imaging is to some extent a residium of an original left-right gradient (or physiological superiority of the left side) of the pre-twin embryo. At least this much of the theory that double monsters and some separate one-egg twins are derived from the two prospective halves of a single individual must therefore be retained.

There are still several unsolved problems connected with mirror imaging in twins. Why do two-egg twins show more extensive reversals in handedness, hair whorl, etc., than do singly-born individuals? Why do one-egg twins show mirror imaging in some and not in other characters? Why do conjoined twins show more extensive mirror imaging than separate one-egg twins? Why do some separate one-egg twins show no mirror imaging and others rather extensive mirror imaging? These and other twinning problems must be solved before we can hope to really satisfactorily understand mirror imaging. It is not our present

purpose, however, to deal with these questions. We shall be content if we have succeeded in showing that mirror imaging in separate one-egg twins is real and that it has in all probability an environmental basis.

SUMMARY

1. Mirror imaging may be said to exist between two individuals when one individual exhibits the normal asymmetry and the other individual reversed asymmetry of a character or system.

2. *Situs inversus viscerum* (s. i. v.) is an extreme expression of asymmetry reversal. The s. i. v. condition is a mirror-image of the normal condition, *situs solitus*.

3. In salmon embryos s. i. v. was found by Komai to occur rarely in single embryos, more frequently in separate one-egg twins, and most frequently in conjoined twins.

4. There is in salmon embryos a strong correlation between s. i. v., underdevelopment and deformity.

5. There is also a strong tendency for s. i. v. to occur in the right component of conjoined twins, suggesting an original left-sided superiority.

6. The close association of one-egg twinning with s. i. v., underdevelopment, and deformity indicate that all of these correlated anomalies are due to depressing factors.

7. S. i. v. in salmon embryos is comparable to reversed asymmetry in echinoderm larvae, which has been induced by growth-depressing agents.

8. Double monsters in cattle show much abnormality and extensive mirror imaging.

9. S. i. v. is extremely rare in single human individuals. When it occurs it is nearly always associated with other structural abnormalities, suggesting that s. i. v. is induced by growth-depressing agents.

10. Only 5 cases of s. i. v. in separate human one-egg twins have been reported, but it is probable that at least partial s. i. v. in such twins may be relatively frequent.

11. S. i. v. is almost the rule in laterally conjoined human twins, is much commoner in the right component, in the smaller component and in the less normal component of such twins.

12. Minor expressions of mirror imaging, such as those in hair whorl, ear form, palm and sole prints, dental irregularities, etc., are

much commoner in conjoined human twins than in separate one-egg twins. Such minor degrees of mirror imaging are correlated with s. i. v.

13. Mirror imaging in handedness, hair whorl, and palm patterns in separate one-egg twins are real and not to be explained away because two-egg twins and singly born individuals also exhibit pseudo-mirroring with respect to these characters.

14. Minor expressions of mirror imaging are regarded as having the same general cause or causes as s. i. v. All reversals of asymmetry are probably due to growth-depressing agents and are therefore environmental.

15. This conclusion, based upon much new evidence, tends partially to discredit the present writer's earlier view that mirror imaging is due to a "third factor, neither strictly genetic nor environmental."

16. This conclusion helps to simplify those methods of twin research that are directed toward determining the exact shares of genetic and environmental factors in determining variance in human characters.

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PROLIFICACY DISTRIBUTION OF WHITE WIVES ACCORDING TO FERTILITY TABLES FOR THE REGISTRATION AREA

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THE drastic decline of the white birth rate in the United States during recent decades has stimulated studies of fertility from several standpoints. Great progress has been made in measuring it accurately in terms of averages (i.e. per 1,000 women) as is shown by the widespread use of standardized birth rates and ratios of children to women, gross and net reproduction rates, replacement indices, and intrinsic rates for births and natural increase. In contrast, much less has been accomplished in determining for various periods of time the proportion of women having specified numbers of children, and hence in showing the extent to which the decline in average fertility has resulted from (1) a rise in the proportion of infertile (childless) wives, and (2) a decline in the proportion of wives bearing several children.

The information now available regarding the prolificacy distribution of wives, i.e. the proportion having 0, 1, 2, 3 and other numbers of children, comes from two types of data. These are (1) the number of births to date as reported by married women in the 1900 and 1910 censuses, or in a few special studies, and (2) the number of legitimate births by order of birth as reported on birth certificates. The census data have been used to excellent advantage by Hill, and Notestein[1]

[1] Hill, Joseph A.: Comparative fecundity of women of native and foreign parentage in the United States. *Journal of the American Statistical Association*, December, 1913, Vol. 13, No. 103, pp. 583-604.

Notestein, Frank W.: The decrease in size of families from 1890 to 1910. *Milbank Memorial Fund Quarterly Bulletin*, October, 1931, Vol. ix, No. 4, pp. 191-199.

but they are available only for a period ending nearly 30 years ago, since the necessary questions were omitted from the 1920 and 1930 census schedules. The few special studies of number of children ever born are limited in scope because of the expense of gathering the data.[2] Births by order of birth have been analyzed intensively by Pearl, primarily from the standpoint of the current population.[3] Outstanding contributions have been made by Lotka and Burks toward a methodology for utilizing these data to describe the prolificacy distribution of a cohort passing through life under exposure to current vital rates.[4] The material which they have published for the birth registration area as a unit is of so much value that it has seemed worth while to the Scripps Foundation to apply their methods to individual States for various periods of time.[5] To do this adequately has required a careful study and evaluation of these methods and of the basic data which must be utilized in carrying them out. This paper presents some of our findings.[6] It is hoped that they may contribute to the intelligent use of order of birth data in determining trends and differentials in prolificacy distribution in the United States.

Just as a life table shows the number of persons dying and the expectation of life at each age in a population exposed throughout its

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[2] One exception may be the "Depression and Health Survey." The schedules contained questions regarding the number of children born alive, but so far no prolificacy distribution tables based on these data have been published.

[3] Pearl, Raymond: Variation in parity of women bearing children in the United States Birth Registration Area in 1930. *Human Biology*, February, 1937, Vol. 9, No. 1, pp. 65-98, and May, 1937, Vol. 9, No. 2, pp. 212-244.

[4] Lotka, Alfred J.: Sterility in American marriages. *Proceedings of the National Academy of Sciences*, January, 1928, Vol. 14, No. 1, pp. 99-109.

Burks, Barbara S.: A statistical method for estimating the distribution of sizes of completed fraternities in a population represented by a random sampling of individuals. *Journal of the American Statistical Association*, December, 1933, Vol. 28, No. 184, pp. 388-394.

[5] An article presenting the State material is in preparation.

[6] During the course of our study Dr. Lotka kindly furnished us with unpublished revisions of his above-mentioned article. He has called our attention to the fact that in the latter the fertility table for all women was applied to the married women "restored to life," whereas in his revision the equivalent of a fertility table for married women is used. The correction raises his percentage of "gross sterility" due to premature death of wives, and lowers by the same amount his percentage of "net sterility." His corrected method is used throughout this paper.

lifetime to the specific mortality rates of the base period in question, so the application of Lotka's and Burks' methods shows the number of wives having zero, one, two or other number of births in a population exposed throughout its lifetime to given specific rates for fertility, mortality, marriage and divorce. The procedure for a given population may be explained briefly as follows:—(1) Multiplying the number of women dying at each year of age according to a life table by the percentage of women married, widowed or divorced in that age period according to the census and adding the products gives the number of women in the life table cohort who become wives.[7] (2) Multiplying the number of women alive at the mid-point of each year of age according to the life table by age specific rates for legitimate first births, second births, third births,[8] etc., adding each series of products, and obtaining the differences between the successive totals, gives the number of wives having one birth, two births, three births, etc. (3) Dividing each of the values in (2) by the total number of life table wives as computed in (1) gives the percentage of wives having one birth, two births, etc. (4) Subtracting the sum of the percentages in (3) from 100 gives the percentage of wives who are childless.[9]

These results are for all wives, including those whose marriage is prematurely broken by divorce, by death of husband, or by their own

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[7] It should be noted that the result obtained by this step is assumed to be the same as the result which would be obtained by using a composite life table based on marriage and divorce rates as well as on death rates. A test of this assumption, based on data available for the United States, indicates the error resulting from it is negligible, for death rates of single women are higher than those for married women but lower than those for widows and divorcees, hence are close to those for all women who have been married.

[8] To illustrate the computation of these rates, the number of legitimate first births to women aged 20-24 (for example), is divided by the number of women aged 20-24 (regardless of marital condition). This rate is then applied to the life table women aged 20½, 21½, 22½, 23½, and 24½.

[9] The number of marriages is greater than the number of wives due to the remarriage of widows and divorcees. Since no allowance is made for remarriage in this method, the result obtained is the percentage of *wives* that are childless rather than the percentage of *marriages*. Obviously the percentage childless is lower for wives than for marriages, and the percentage with several children is higher, the size of the differences depending on the extent to which remarriage occurs.

death.[10] The prolificacy distribution of wives whose marriage is not broken may be computed by "restoring" such marriages, and assuming that they have the same fertility as other marriages.[11] The process utilized by the writers involves for each age period the equivalent of the following steps: (1) Multiply the number of women living at the mid-point of the age period and the number dying during the age period according to the life table by the percentages of women in the age period who are married, widowed and divorced according to the census. (2) Add the numbers of women married, widowed, or divorced living at the mid-point of the age period and the numbers of women dying married, widowed, or divorced prior to the mid-point of the age period and divide the total by the number of women married at the mid-point of the age period. (3) Use the quotient thus obtained for the age period as a multiplier for the number of wives of that age having one birth, two births, etc., as computed in step (2) of the preceding outline, and continue as before.

Before discussing the results it should be pointed out that in step (2) above there are two modifications of Lotka's procedure: (1) Deaths of widows and divorcees are taken into account here, *all* marriages broken prematurely by divorce or by death of husband being "restored," not merely those in which the divorcee or widow lives through the remainder of the child-bearing period. This modification tends to reduce slightly the percentage of unbroken marriages which are childless and to increase by smaller amounts the percentages of unbroken marriages having one, two or other number of births. (2) In "restoring" marriages broken prematurely by death of wife, Lotka's procedure was to compute a population for each one-year age period, sum these by five-year age periods, and apply the age specific birth rates used in the computations for all wives. His formula for this population is:

$$p(a + \tfrac{1}{2}) + \frac{1}{u(a)} \sum_{o}^{a} u(x)d(x) \qquad \text{(i)}$$

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[10] A marriage is assumed to be broken "prematurely" if at the time of the break the wife is young enough to have a child. For practical purposes 50 may be taken as the upper age limit, since the birth rate to women over 50 is negligible.

[11] For each "wife whose marriage is not broken" there is an "unbroken marriage" and vice versa, hence these two terms may be used interchangeably in discussing prolificacy distribution.

Where $p(a + \frac{1}{2})$ is the number of survivors at exact age $(a + \frac{1}{2})$,
$u(a)$ is the average percent married in the interval from exact age (a) up to but not including exact age $(a + 1)$
$\sum_{\bullet}^{a}$ is the summation from $x = 0$ to $x =$ the interval from exact age (a) up to but not including exact age $(a + 1)$
$u(x)d(x)$ is the number of deaths married in the interval from exact age (x) up to but including exact age $(x + 1)$

It appears to the writers that the same fertility rates cannot properly be applied to the number of women shown in the two terms of the formula. Some of the women in the first term will die between exact age $(a + \frac{1}{2})$ and exact age $(a + 1)$ but all of those in the second term will live until exact age $(a + 1)$ and hence should rank higher in fertility. The specific birth rates to be applied are obtained by dividing the number of births to women in the age interval (x) to $(x + 5)$ by the number of women in the age interval (x) to $(x + 5)$, and strictly speaking should be applied only to a population of this type. Within the child-bearing period, however, the number of women at exact age $(x + \frac{1}{2})$ and in the age interval (x) to $(x + 1)$ are considered identical in a conventional life table, the value for $l_{x+\frac{1}{2}}$ equaling that for L_x. Similarly, the number of deaths prior to exact age $(x + \frac{1}{2})$ equals the number of deaths prior to the age interval $(x - \frac{1}{2})$ to $(x + \frac{1}{2})$. The first term of Formula (i), therefore, appears correct for the purpose in mind, but it is evident that the second term should be changed as follows:

$$p(a + \tfrac{1}{2}) + \frac{1}{u(a)} \sum_{o}^{a-\frac{1}{2}} u(x)d(x) \qquad \text{(ii)}$$

where $\sum_{o}^{a-\frac{1}{2}}$ is the summation from $x = o$ to $x =$ the interval from exact age $(a - \frac{1}{2})$ up to but not including exact age $(a + \frac{1}{2})$.

Formula (ii) has been used in computations for this paper.*

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* In correspondence with the writers Mr. Mortimer Spiegelman suggested the formula

$$p(a + \tfrac{1}{2}) + \frac{1}{u(a)} \sum_{o}^{a-1} u(x)d(x) + \tfrac{1}{2}d(a) \qquad \text{(iii)}$$

which he simplified as

$$p(a) + \frac{1}{u(a)} \sum_{o}^{a-1} u(x)d(x) \qquad \text{(iv)}$$

But (iv) is incorrect for each age up to and including the youngest age at which deaths married occur, because the second term then becomes zero. On this account it appears preferable to simplify (iii) as is done in (ii).

From the brief description of method given above, it is apparent that the accuracy of the results obtained by applying it depends on the accuracy and completeness of the data for births by legitimacy, order of birth, and age of mother, and of the data for women by age and marital condition.[12] Some of the more important shortcomings of these data will now be considered, and an attempt made to evaluate their effect. Most of the discussion will relate to the 1920 period, because of the wide use of Lotka's results for that year. Attention will be centered on the proportion of marriages childless and the proportion fertile, since the problems arising in determining these items appear more important on the whole than those arising in determining the distribution of fertile marriages by specific numbers of births.

I. THE COMPLETENESS OF BIRTH REGISTRATION

Probably the most serious criticism of the above-mentioned data is that registered births do not include all births. It has long been recognized that an important proportion of births in the registration area are not recorded, though there is some variation between the size of the different estimates of the omissions. According to Miss Foudray only 92.64 percent of the white births in 1919 were recorded in the current area.[13] Following a procedure similar in part to that of Miss Foudray, the writers have estimated that 93.7 percent of the white births were registered in the year ending March 1, 1930, but slightly lower proporions have been obtained by Dorn and Smith.[14] In no case known to the

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[12] The fact that death registration is not complete in all registration states increases the number of survivors at each age in the life tables used in these computations. In consequence, the number of women marrying is overstated somewhat, as is also the number of women having specified numbers of children, the overstatement among the latter varying directly but slightly with the ordinal number of children. The effect on prolificacy is unimportant, however, since some of these errors are partially compensatory.

[13] United States Bureau of the Census: United States Abridged Life Tables, 1919-20. Prepared by Elbertie Foudray. *Washington*, Government Printing Office, 1923. The results of her 1930 study have not been published.

[14] Whelpton, P. K.: The completeness of birth registration in the United States. *Journal of the American Statistical Association*, June, 1934, Vol. 29, No. 186, pp. 125-136.

Smith, T. Lynn: Rural-urban difference in the completeness of birth registration. *Social Forces*, March, 1936, Vol. 14, No. 3, pp. 368-372.

Dorn, Harold F.: The completeness of birth registration in the United States. (Unpublished.) His table, "Estimated percentages of births registered, 1925-1928,"

writers has an investigation been made of the effect of legitimacy, order of birth, or age of mother on the completeness of registration. Hedrich, Collinson, and Rhoades found that the completeness of registration varied directly with economic status and/or amount of schooling in Georgia in 1934 but that these factors did not appear to be interrelated in Maryland in 1937.[15] Coupling the Georgia findings with the evidence that illegitimacy and fertility vary inversely with economic status and amount of schooling, indicates that omissions may be less numerous relatively among legitimate than illegitimate births, and among legitimate births of low order than those of high order, at least in parts of the Birth Registration Area. With the illegitimacy rate as low as it is, a large differential in omissions for legitimate and illegitimate births would raise the percent registered for legitimate births only a little above that for all births.[16] In contrast, since no one order of legitimate births constitutes a majority of all legitimate births, the degree of completeness of recording low and high order births may differ significantly from that for all births. Until these relationships are established more definitely, however, it seems necessary to use the correction factor for under registration of all births in adjusting legitimate births of each order.

If only registered births are used in the computations for 1920, and if Lotka's assumptions are made regarding the data for illegitimate births and the proportion of women single (both of which will be discussed later) 17.8 percent of all wives and 12.6 percent of wives with unbroken marriages are infertile. In contrast, if it is assumed that 92.64 percent of the births were registered, the former proportion is reduced by over one-fourth to 11.2 and the latter by more than one-half to 5.8. (See Table 1 and Chart 1, lines A and B.) Similarly, making this allowance

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appears on page 29 of Lorimer, Frank: Suggestions on population studies for state planning boards. *National Resources Committee, Washington, D. C.*, 1938. (Mimeographed.)

[15] Hedrich, A. W., Collinson, John, and Rhoades, F. D.: Results of birth tests by several methods in the same area. Paper read at the 67th Annual Meeting of the American Public Health Association, October, 1938. The term "intelligence" was used in this paper, but for most of the persons interviewed it was determined by "highest grade of school completed."

[16] For example, if 92.64 percent of all white births were registered in 1920 as is assumed here, there may have been a differential of 10 percent between legitimate and illegitimate births. This would mean 92.77 percent for the former and 82.77 percent for the latter on the basis of 98.68 percent of all white births being registered as legitimate in that year.

TABLE 1

Percentage of white wives fertile and infertile according to current fertility and life tables, with various adjustments of basic data; 1920, 1919-21 and 1929-31[1]

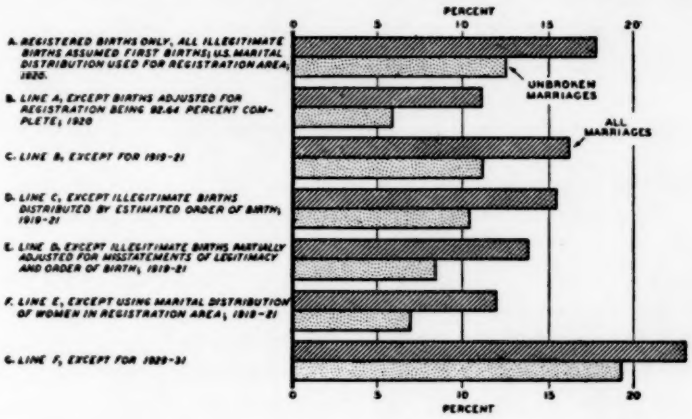
YEAR AGE AT MARRIAGE ADJUSTMENT OF BASIC DATA	PERCENT OF ALL WIVES		PERCENT OF INFERTILITY DUE TO PREMATURE			PERCENT OF WIVES WITH UNBROKEN MARRIAGES	
			Death of				
	Infertile	Fertile	Wife	Husband	Divorce	Infertile	Fertile
I. *Wives marrying at all ages*							
A. Registered births only; all illegitimate births assumed 1st births; U. S. marital distribution used for Registration Area; 1920[3]	17.8	82.2	2.2	2.1	.9	12.6	87.4
B. Line A, except births adjusted for registration being 92.64 percent complete; 1920	11.2	88.8	2.3	2.2	.9	5.8	94.2
C. Line B, except for 1919-21	16.2	83.8	2.2	2.1	.8	11.1	88.9
D. Line C, except illegitimate births distributed by estimated order of birth; 1919-21[3]	15.5	84.5	2.2	2.1	.9	10.3	89.7
E. Line D, except illegitimate births partially adjusted for misstatements of legitimacy and order of birth; 1919-21[3]	13.9	86.1	2.3	2.0	.9	8.7	91.3
F. Line E, except using marital distribution of women in Registration Area; 1919-21[3]	12.0	88.0	2.2	2.0	.9	6.9	93.1
G. Line F, except for 1929-31[4]	23.1	76.9	1.1	1.2	1.4	19.4	80.6
II. *Wives marrying under 45*							
H. Adjustments of line E; 1919-21	11.3	88.7	2.4	2.2	.9	5.8	94.2
I. Adjustments of line F; 1919-21	10.3	89.7	2.2	2.1	.9	5.1	94.9
J. Adjustments of line F; 1929-31[4]	22.2	77.8	1.1	1.3	1.4	18.4	81.6

NOTE:—For notes for Table 1 see next page.

for incomplete registration increases the proportion of unbroken marriages fertile by nearly one-twelfth (from 87.4 to 94.2) and increases the proportion having one, two, three, or other number of children in the same degree. The changes in the precentages for infertile wives may be too small by as much as 1.0, and perhaps by as much as 3.0 or 4.0, since the method followed by Miss Foudray in obtaining the estimate of 92.64 percent for the completeness of birth registration appears more likely to give a percentage which is too high than one which is too low.

CHART I

PERCENTAGE OF WHITE WIVES CHILDLESS, BASED ON CURRENT FERTILITY AND LIFE TABLES, WITH VARIOUS ADJUSTMENTS OF BASIC DATA; 1920, 1919-21 AND 1929-31 [1]


PERCENT
0
5
10
15
20
A. REGISTERED BIRTHS ONLY, ALL ILLEGITIMATE BIRTHS ASSUMED FIRST BIRTHS; U.S. MARITAL DISTRIBUTION USED FOR REGISTRATION AREA; 1920.
B. LINE A, EXCEPT BIRTHS ADJUSTED FOR REGISTRATION BEING 92.64 PERCENT COMPLETE; 1920
C. LINE B, EXCEPT FOR 1919-21
D. LINE C, EXCEPT ILLEGITIMATE BIRTHS DISTRIBUTED BY ESTIMATED ORDER OF BIRTH; 1919-21
E. LINE D, EXCEPT ILLEGITIMATE BIRTHS PARTIALLY ADJUSTED FOR MISSTATEMENTS OF LEGITIMACY AND ORDER OF BIRTH; 1919-21
F. LINE E, EXCEPT USING MARITAL DISTRIBUTION OF WOMEN IN REGISTRATION AREA; 1919-21
G. LINE F, EXCEPT FOR 1929-31
UNBROKEN MARRIAGES
ALL MARRIAGES
0
5
10
15
20
PERCENT

[1] BASED ON TABLE I

CENTRAL STATISTICAL BOARD

SOURCE: SCRIPPS FOUNDATION

NOTES FOR TABLE I.

[1] The fertility tables are for the current Birth Registration Area (omitting Maine in 1920 and 1919-21). The U. S. Abridged Life Tables, 1919-20 with interpolated values prepared by the Metropolitan Life Insurance Company are used in computations for 1920 and 1919-21, and the U. S. Life Tables, 1930, in those for 1929-31.

[2] From revised table furnished by Dr. Lotka, but using Formula (ii) instead of Formula (i), and "restoring" all women who died after marrying. (See text.)

[3] See text for an explanation of the adjustment.

[4] Assuming birth registration was 93.7 percent complete in 1929-31.

In interpreting the above results, it should be kept in mind that the use of the same correction for legitimate births of all orders to mothers of all ages probably leads to overstating the proportions of fertile wives and marriages and understating those of childless wives and marriages, perhaps by as much as 1.0, but almost certainly by not over 3.0. This tends to compensate for the use of too high a percentage of births registered, mentioned above. Within the fertile group, if the completeness of registration declines uniformly as order of birth increases, the use of the same factor for all birth orders will tend to overstate slightly but uniformly the proportions of wives and marriages having each number of births except the highest. But if the decline in completeness of registration is not significant until after the third or fourth birth, the proportions with one to three or four births will be correct and only subsequent orders will be overstated. In any case, however, the relative error for each order from one to ten probably is not important.

II. THE NORMALITY OF 1920 BIRTH RATES BY ORDER OF BIRTH

In utilizing life tables, fertility tables, or prolificacy tables for a given period, it is important to know to what extent the period is typical. Life tables for 1918, for example, must be interpreted in light of the fact that, as a result of the influenza epidemic, death rates were much higher in that year than in any other year over a long period.[17] During the five years from 1918 to 1922 white birth rates per 1000 persons in the current birth registration area fluctuated somewhat erratically, chiefly as a result of the mobilization and demobilization of a large United States Army. (See Table 2.) When crude rates by order of birth for each year from 1917 to 1923 are compared with averages for three, five, and seven years centering on 1920,[18] no single year is found to be more typical of the period than is 1920; nevertheless, the fact that the first birth rate of that year is 5.7 percent above the seven-year average is a decided objection to its use as a base period for a prolificacy distribution table. Rates for the three-year period 1919-21 resemble those for the seven-year period much more closely than do rates for 1920, hence 1919-21 should be superior to 1920 as a bench mark from which to judge

[17] Sydenstricker, Edgar: Health and Environment, New York, McGraw-Hill Book Company, 1933, pp. 151-155.

[18] Because rates are based on the census population it is desirable to utilize periods centering on census years.

TABLE 2

*White births, by order of birth, per 1000 persons in the current Birth Registration Area; 1917-23**

	TOTAL	1ST	2ND	3RD	4TH	5TH	6TH	7TH TO 9TH	10TH & HIGHER
1917	24.60	6.73	5.28	3.82	2.67	1.90	1.39	2.10	.71
1918	24.43	6.75	5.22	3.78	2.66	1.87	1.36	2.09	.70
1919	22.05	5.35	4.96	3.54	2.52	1.76	1.27	1.99	.66
1920	23.46	6.88	4.87	3.52	2.50	1.76	1.26	1.98	.69
1921	24.04	7.20	4.88	3.56	2.56	1.80	1.30	2.04	.70
1922	22.24	6.44	4.97	3.24	2.31	1.63	1.17	1.84	.64
1923	22.13	6.23	5.07	3.28	2.28	1.62	1.17	1.83	.65
1920	23.46	6.88	4.87	3.52	2.50	1.76	1.26	1.98	.69
1919-21	23.18	6.48	4.90	3.54	2.53	1.77	1.27	2.00	.68
1918-22	23.24	6.52	4.98	3.53	2.51	1.76	1.27	1.99	.68
1917-23	23.28	6.51	5.04	3.54	2.50	1.76	1.27	1.98	.68

* Computed from data in current volumes of "Birth Statistics," excluding states which do not give births by order, viz. Massachusetts, 1917-23; Mississippi, 1921; Rhode Island, 1921-22; and Delaware, 1923, but distributing births with order unknown in other registration states. No allowance is made for incomplete registration.

trends. The use of 1917-23 as a base period would be only very slightly better, and would involve dealing with many more data. In consequence, 1919-21 was chosen as the base for this study.

Since most wives are fertile, the above change in the base period affects the ratio of fertile wives to all wives in approximately the same degree that it affects the crude first birth rate. Thus, the proportion of all wives fertile for 1919-21 (83.8) is lower than that for 1920 (88.8) by 5.6 percent, whereas the crude first birth rate for 1919-21 (6.48) is lower than that for 1920 (6.88) by 5.8 percent. (See Table 1 and Chart 1, lines B and C, and Table 2.) But the proportion of infertile wives computed by subtracting the proportion of fertile wives from 100 percent is a residual in the sense used by Truesdell[19] and fluctuates much more

[19] Truesdell, Leon E.: Residual relationships and velocity of change as pitfalls in the field of statistical forecasting. *Journal of the American Statistical Association*, June, 1938, Vol. 33, pp. 373-379.

violently. Changing the base period from 1920 to 1919-21 raises the percentage infertile by nearly one-half (from 11.2 to 16.2) for all wives, and almost doubles the percentage (from 5.8 to 11.1) for those with unbroken marriages.

Space does not permit a detailed consideration here of the effect of changes in the base period on the proportion of wives having one, two, or other numbers of children, but it may be pointed out that the principle is the same. For example, wives having one birth are computed by subtracting those having two or more births from those having one or more births, the two latter quantities varying in approximately the same ratio as the second and first birth rates. The difference between them, however, usually is much smaller than either term itself, hence fluctuates in much greater degree. In other words, a comparatively small decrease from one period to another in the birth rate for second children (rates for first and third children remaining unchanged) will cause a large increase in the proportion of wives having one birth and a large decrease in the proportion of wives having two births as computed by the method under discussion. Values in a prolificacy distribution table, therefore, will fluctuate much more from one base period to another than will values in a fertility table or a life table.

III. THE CLASSIFICATION OF BIRTHS BY LEGITIMACY

None of the published or unpublished tabulations made to date by the Division of Vital Statistics throws any light on the number of first births which are legitimate or the number of illegitimate births which are first births. According to Lotka "we may without serious error, put down all illegitimate births as first births. For, in the first place a large proportion of illegitimate births are actually first births. Secondly, of those illegitimate births that may not be first births, those by married women will in most cases be reported as legitimate; on the other hand, illegitimate births out of wedlock will, in the majority of cases, be reported as *first* births even if not such in fact. Hence we shall, without serious error, deduct *all* illegitimate births from the total first births to obtain the legitimate first births."[20] Although Lotka's suggestion may be the best one to follow, an attempt to evaluate it is desirable.

Judging from the scanty data which are available and from discussions with social workers, an important proportion of illegitimate births

[20] *Op. cit.*, p. 100

are not reported as first births. The only data for large groups known to the writers come from studies made by the Children's Bureau in Baltimore and from special tabulations of Baltimore and of Maryland counties by the City and State Bureaus of Vital Statistics. In Baltimore 82.3 percent of the registered illegitimate births to white women in 1915 were first births and 17.7 percent second or higher order;[21] in 1935 the corresponding percentages were 77.3 and 22.7 according to the Children's Bureau data,[22] and 74.9 and 25.1 respectively according to the State Bureau data.[23] For the Maryland counties in 1930 the percentages were 71.2 and 28.8, respectively. From these Baltimore data and the percent of all white births by age of mother which were first births in the city, it is estimated that the proportion of registered illegitimate births in 1915 which were first births was 100 percent at ages 10-14, 95 percent at ages 15-19, 90 percent at ages 20-24, 65 percent at ages 25-29, 25 percent at ages 30-34, and 10 percent at ages 35-39.[24] To assume that the percentages for the entire birth registration area in 1919-21 are similar to these for Baltimore in 1915 is open to question, but is believed to be partially justified by the similarity which exists between other percentages for these two areas, in particular the percent of all white births by age of mother which are first births. In view of the relation between the later data for Baltimore and the Maryland counties, however, it is probable that the above percentages for first births are too high.

Changing from the assumption that all registered illegitimate births are first births to the assumption that the above estimates are approximately correct reduces the 1919-21 proportion infertile from 16.2 to 15.5 for all marriages and from 11.1 to 10.3 for unbroken marriages. (See Table 1 and Chart 1, lines C and D.) For the reason stated above, however, these reductions probably leave the percent infertile still too high.

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[21] Rochester, Anna: Infant mortality: results of a field study in Baltimore Maryland, based on births in one year. *Children's Bureau Publication*, No. 119, *Washington*, Government Printing Office, 1923, Table 198, p. 373.

[22] Unpublished data furnished by Dr. Elizabeth C. Tandy of the Children's Bureau. Studies of a similar nature were made by the Bureau in several cities but in none of them do illegitimate births appear to have been tabulated by order in *all* births.

[23] Unpublished data furnished by Dr. A. W. Hedrich, Chief of the Bureau of Vital Statistics of Maryland.

[24] A statement explaining the process followed in estimating these percentages and showing the data utilized may be obtained by writing to Scripps Foundation, Oxford, Ohio.

Two other problems regarding illegitimate births remain to be considered; namely, the false reporting of illegitimate births as legitimate, and the consideration of illegitimate as well as legitimate births in reporting the birth order of the latter on the birth certificate. Since most illegitimate births are first births, attention will be focused on women registering (1) an illegitimate first birth as such followed by (a) a legitimate birth as a first birth, or (b) a legitimate birth as a second birth, and (2) an illegitimate first birth as legitimate followed by (a) no birth, (b) a legitimate birth as a first birth, or (c) a legitimate birth as a second birth. It is evident that the use of unadjusted data for any one of these groups except (1a) will lead to a distortion of a prolificacy distribution table for marriages.[25] Is it possible to measure this distortion or to adjust the data?

In the case of group (1b), women registering an illegitimate first birth and a legitimate second birth, it is belived that a conservative estimate can be obtained of their legitimate second births in 1919-21. The process and the assumptions involved are as follows:

(1) The proportion of women having second births in 1919-21 whose first birth was registered as illegitimate is assumed to equal the proportion of first births in 1917-18 which were illegitimate. The latter years are chosen because they average two and one-half years prior to 1919-21, two and one-half years being approximately the average difference in age at that time between mothers having first and second births. This is a conservative assumption because there are several reasons for expecting more second births to women whose first birth is illegitimate than to those whose first birth is legitimate. The former are younger and on the whole have a lower socio-economic status than the latter.[26] There is evidence that (a) fertility tends to rise with a decline in age at bearing first child and/or in socio-economic status,[27] and (b) the interval between births tends to decrease as fertility increases.

(2) These estimated second births in 1919-21 to mothers whose first birth was illegitimate are assumed to have a distribution by age of

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[25] In this discussion it is assumed that marital status is reported correctly to the census enumerator, a matter which will be considered later.

[26] In Baltimore in 1915 the average age of all mothers having a first birth was approximately 23.2 years and of those mothers having an illegitimate first birth 21.0 years, Rochester, Anna, *op. cit.*

[27] Notestein, Frank W., *op. cit.*

mother resembling the average distribution of first and second births in 1919-21. The average distribution is used instead of the distribution for second births because, as mentioned above, women having an illegitimate first birth are younger than those having a legitimate first birth.

(3) Subtracting from the second births thus obtained all the second births registered as illegitimate gives the legitimate second births which were first legitimate births in each age group in 1919-21.[28] This is also a conservative assumption because it is certain that some of the illegitimate second births occur to women whose first birth was registered as legitimate.

(4) The results in (3) are reduced by 25 percent to allow for the fact that some legitimate second births are registered as legitimate first births, the women concealing the birth of a previous illegitimate child. This percentage is chosen arbitrarily in the absence of statistical data, but in the opinion of certain social workers it is sufficiently large to make a minimum adjustment and obtain results which are between the actual situation and the results based on the unadjusted data.

Revising the number of legitimate first births by means of the figures obtained by the process just described reduces the proportion infertile for all wives from 15.5 to 13.9 and for unbroken marriages from 10.3 to 8.7. (See Table 1 and Chart 1, lines D and E.) In like manner, the proportion of unbroken marriages which are fertile is increased from 89.7 to 91.3.

By a similar process an allowance can be made for first legitimate births which are third or higher order births. The size of the adjustment becomes increasingly small, however, and was not included in this study.

Considering the second of the two groups previously described (the women who report an illegitimate birth as legitimate), it is well known that the social attitudes toward illegitimacy lead to such misrepresentation, and to single women being enumerated as married in the census. It is almost certain that the former constitute a larger proportion of all legitimate births than the latter constitute of all married women, but unfortunately, data are almost non-existent regarding the numbers involved in either case. Here as before, the most helpful information for births comes from intensive studies by the Children's Bureau. Among 19,462 births (mostly white) registered in Boston in 1914 there were

[28] The proportions of illegitimate births which are second, third, and higher order are estimated by the same process as the proportions which are first births. See preceding paragraphs.

752 births registered as illegitimate, but an additional 95 births registered as legitimate were found to have been illegitimate.[29] Among 10,563 white births in Baltimore during 1935, of which 216 were registered as illegitimate, the Bureau found that 15 registered as legitimate actually were illegitimate.[30] The maximum error in the computed proportions of fertile and childless wives probably occurs when the illegitimate births falsely returned as legitimate are first births, and the mothers either have no subsequent births, or have legitimate births which they register as first births. In contrast, no error in the proportion of wives fertile and childless occurs if there are illegitimate births falsely reported as legitimate and followed by legitimate second births which are reported in the correct order for all births, although such cases do cause errors in the proportions with two or larger numbers of births. The falsifications of legitimacy discovered in Boston, therefore, could not have increased the number of legitimate first births by much more than two percent, nor those in Baltimore by as much as 0.4 percent. They would fail to reach these limits by an amount depending on the number of cases of the second type, and on the number of single women who have an illegitimate child but who are enumerated as married. Records of social agencies show that a large proportion of women having an illegitimate first birth marry the father of the child, hence would undoubtedly register the next birth as second legitimate, and also that if they register the first as legitimate, it is because they are passing as married among most people with whom they come in contact and would probably be so enumerated in a census. On the other hand, it is very unlikely that all the illegitimate births registered as legitimate were discovered in the Baltimore and Boston studies. How the successfully falsified compare in number with those discovered must remain a matter of speculation until additional facts are obtained.

If the proportion of first births registered as legitimate, but actually illegitimate, is twice that discovered in Boston and Baltimore, if only half of these births are followed by a legitimate second birth registered as such, and if no single women with illegitimate children are enumerated as married, the proportion of fertile and childless wives would approxi-

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[29] Lundberg, Emma O., and Lenroot, Katherine F.: Illegitimacy as a child-welfare problem, Part 2, *Children's Bureau Publication* No. 75, *Washington*, Government Printing Office, 1921, p. 86.

[30] Tandy, Elizabeth C., *ibid.*

mate those in line D of Table 1. It seems more likely to the writers, however, that the true values are between lines D and E.

The foregoing discussion assumes that all illegitimate births should be omitted in computing the prolificacy distribution of wives. It might be argued, however, that the illegitimate births to parents who subsequently marry each other should be included with the legitimate births; in other words, that the fact of marriage rather than the date of marriage should be the decisive factor. Since such cases would include an important proportion of illegitimate births, the adoption of this point of view would lessen greatly the need to adjust for misstatements of legitimacy and for reporting legitimate births in their order among all births instead of legitimate births. The figures in line E, Table I, should then describe the actual situation more closely.

IV. MARRIAGES OF WOMEN OF CHILD-BEARING AGE

In obtaining the proportion of wives that are infertile, Lotka allowed for the influence of divorce and of death but not of age at marriage. Many marriages broken by divorce or death have already been fertile, but no marriage contracted after the bride has passed the child-bearing period can be fertile. It is desirable, therefore, to extend Lotka's method by excluding marriages in which the bride is single and too old to have a child, and compute the prolificacy distribution of wives whose first marriage occurs when they are sufficiently young to be fertile. Results for unbroken marriages of such women are needed for accurate comparison with the figures for prolificacy distribution obtained by Notestein from the 1910 census data.

The procedure developed by the writers involves one new step. As was indicated earlier in describing Lotka's method, the total number of infertile wives in a life table population is obtained by subtracting the number of women bearing one or more legitimate children from the number of women dying married, widowed or divorced. Since life tables are not available by marital condition for most parts of the United States, the latter number is the life table cohort minus the summation of deaths at each age multiplied by the percent single at that age as estimated from data in the census. If the percent single at exact age 45 is used at each older age, the summation includes only women marrying

before 45.[31] Substituting this value for the other in the appropriate places gives the desired values for prolificacy distribution of all wives marrying before 45 and of those with unbroken marriages.

Under conditions of 1919-21 and also of 1929-31 the inclusion of marriages contracted by women after age 45 increases the proportion of wives infertile by about the same amount as the breaking of marriages by the premature death of the wife. For the former period the difference amounts to between 1.7 and 2.6, depending on the percentages used for marital status. (Compare lines E, F, H, and I of Table 1.) Since birth rates to married women declined sharply from 1919-21 to 1929-31, the effect of postponing marriage was greatly reduced, the difference between lines G and J being only 0.9.

In a similar manner, an allowance could be made for wives who marry men too old to participate in reproduction. Such computations do not appear to be worth while, however, since (1) there is no fairly definite and uniform upper age limit to the fertility of men as there is of women and (2) the indications are that the upper age limit usually is considerably higher for men than for women, hence that the biological effect on fertility of late marriages for men is small.

V. MARITAL CONDITION

One more problem arising from shortcomings in the basic data is worth mentioning, namely, obtaining for each year of age the proportion of women who are single, married, widowed, and divorced in the Birth Registration Area. Percentages for the United States are published in the 1920 and 1930 censuses for white women of each year of age from 15 to 34, and by 5 or 10 year age periods thereafter. For each state, however, figures are published only by 5 or 10 year age periods. Percentages for white women of each year of age in the Birth Registration Area must be estimated from these data.

The accuracy of the published returns on marital status at each year of age is also open to question. The probability that many single women with illegitimate children are enumerated as married has already been pointed out. Furthermore, it seems unlikely that the percent of women single at each year of age from 28 to 33 varied as greatly in 1920 as

[31] No exact age can be set prior to which a woman's marriage may be fertile and after which it must be infertile, but the first birth rate at ages 45 or over is so low that for all practical purposes this age may be taken as the dividing line.

reported in the census, viz. 20.2, 17.5, 18.5, 14.5, 14.7, and 12.7. Such discrepancies are believed to be associated with tendencies for the age of women past the twenties, as reported to the census enumerator, to cluster unduly on years ending in 0, 5, and other even numbers, and for this clustering to occur in greater degree for single women than for married women since the former are more likely than the latter to be employed away from home and hence less likely to report personally to the enumerator. That these and other discrepancies do not operate entirely within 5 or 10 year age periods is probable, but that they carry over on a large scale from one such period to another does not appear to have been proved conclusively, nor measured.

Among the courses which may be followed in estimating the desired percentages for individual ages from those for the census are periods are: (1) Apply a smoothing interpolation formula to the United States data by 5 or 10 year age periods and assume the results are valid for the Birth Registration Area. This was Lotka's procedure in his revised work, Jenkins' formula being utilized. Multiplying the population by the percents thus secured and adding the products by census age groups gives amounts which not only differ widely from the census figures, but differ in the same direction for consecutive age groups. (2) Apply the same smoothing formula to data for the Birth Registration Area. (3) Distribute by 1 year ages the data for each 5 or 10 year age period shown for the Birth Registration Area by the Census, smoothing the series as much as possible along a free hand curve without causing differences between the official figures for 5 year and 10 year age periods and the sums of the corresponding 1 year age figures. The third was utilized by the writers.

Changing from method (1) to (3) affects the results significantly for 1919-21. Percentages of infertility are lowered by about 1/7 (from 13.9 to 12.0) for all wives and by 1/11 (from 11.3 to 10.3) for wives married under 45. Similarly the percentage infertile is lowered by nearly 1/5 (from 8.7 to 6.9) for all unbroken marriages, and by over 1/9 (from 5.8 to 5.1) for those of women under 45. (See Table 1, lines E, F, H, and I.) Interestingly enough, if the second method had been used the change in results would have been even greater. Applying the Jenkins smoothing formula to the 1920 census count of women by marital condition in the Birth Registration Area instead of the United States would decrease the number of "deaths married" by over 2.5 percent. Since the computed percent of wives who are fertile varies

in inverse ratio to the number of "deaths married", this percent would increase (hence the percent of infertile wives would decrease) by over 2.5. The adjustment made between lines E and F (or H and I) is in the right direction, but may not be sufficiently large.

VI. PROLIFICACY DISTRIBUTION OF FERTILE WIVES

Let us broaden the discussion now and consider also the distribution of the fertile wives by specific number of births. This is shown in Table 3 and Chart 2 for the current birth registration area under con-

TABLE 3

*Prolificacy distribution of white wives according to current fertility and life tables; all wives and unbroken marriages with bride under 45: 1919-21 and 1929-31**

	NUMBER OF BIRTHS											
	0	1	2	3	4	5	6	7	8	9	10 & 11	12 & over
All Wives												
1919-21	12.0	20.6	17.7	13.8	10.2	6.8	5.0	3.8	3.1	2.3	2.9	1.8
1929-31	23.1	20.0	19.4	12.1	7.7	4.9	3.6	2.4	2.0	1.5	2.0	1.3
Unbroken Marriages with Bride under 45												
1919-21	5.1	21.1	18.6	14.8	11.0	7.5	5.6	4.3	3.6	2.7	3.5	2.2
1929-31	18.4	20.6	20.3	12.9	8.3	5.3	3.9	2.7	2.2	1.6	2.3	1.5

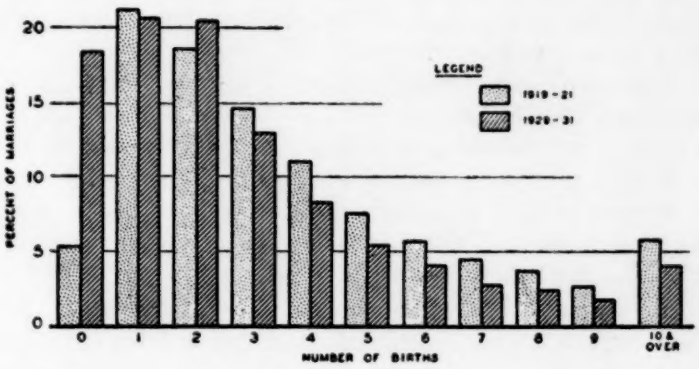
* Comparable with lines F, G, I, and J of Table 1. Births in the registration area adjusted for recording being 92.64 percent complete in 1919-21 and 93.70 percent in 1929-31; illegitimate births are distributed by estimated order of birth and partially adjusted for misreporting of legitimacy and order; the census numbers of women in the Birth Registration Area by marital condition in each 5 or 10 year age period are distributed by 1 year periods. In computing these data, the life tables used for 1919-21 are those in U. S. Life Tables, 1930, prepared by the Bureau of the Census, but the results are the same as in Table 1.

ditions of 1919-21 and 1929-31, the computations being based on registered births adjusted for incomplete registration, misstatements of legitimacy and order, and on the writers' interpolation for marital condition. Several matters stand out clearly according to these figures. (1) Comparing the results for 1929-31 with those for 1919-21, the proportion of wives who are infertile is nearly twice as large for all wives, and

more than three times as large for those with unbroken marriages contracted under age 45. In contrast, the proportions with 4, 5 or other larger numbers of children are lower by between one-fourth and two-fifths. The differences would be more striking if no states had been added to the area between the two periods. (2) The effect on fertility distribution of broken or postponed marriages was somewhat less as of 1929-31 than as of 1919-21. (3) Slightly over one-fifth of all wives had one birth and nearly one-fifth had two births under conditions of both periods. These fractions were about the same for unbroken mar-

CHART 2

PROLIFICACY DISTRIBUTION OF UNBROKEN WHITE MARRIAGES (BRIDE UNDER 45) BASED ON CURRENT FERTILITY AND LIFE TABLES, 1919-21 AND 1929-31 [1]



[1] BASED ON TABLE III

CENTRAL STATISTICAL BOARD

SOURCE: SCRIPPS FOUNDATION

riages with bride under 45. (4) Less than 55 percent of the unbroken marriages (bride under 45) as of 1919-21 and barely 40 percent of such marriages as of 1929-31 could contribute to population growth, i. e. had three or more births. Considering these third or higher order births, approximately 80 percent came from marriages with five or more births, although the latter were barely 29 percent of all marriages as of 1919-21 and 19 percent of all marriages as of 1929-31.

Whether the above statements are essentially correct, or whether the inaccuracies in the basic data or the inadequateness of the adjust-

ments are so great as to make them unreliable is a question which may well be raised. While it should be clear from the preceding discussions that the adjustments are not exact, there is evidence that they improve the data, and that the shortcomings of one are partially offset by shortcomings of others in the opposite direction. Combining the opinions stated in each section, it is the judgment of the writers that the actual percentages of infertile wives under the conditions of the base years is within 3.0 and probably within 2.0 of the values shown in the "0" column of Table 3. This maximum difference is small absolutely, but large relatively, especially in comparison with 5.4, the percent as of 1919-21 for unbroken marriages with bride under 45. Obviously, the percentage of fertile wives will have the same numerical margin of error, but since over 75 percent of wives are fertile according to Table 3, a variation of 2.0 or 3.0 points is of no importance relatively. It is believed that this error is distributed more or less proportionally among the various fertile birth orders, and that it is less than 1/20, possibly less than 1/30, at any order under 10.

The conclusions seem justified, therefore, that the values for wives in a prolificacy distribution table computed from data for the Birth Registration Area adjusted as above (1) are quite reliable for wives with from 1 to 10 children, but (2) are only approximate for infertile wives and must be used with caution. Statements regarding distribution and trend based on Table 3 should be essentially correct with the possible exception of those relating to infertile wives.

In order that prolificacy tables like those under discussion may be properly interpreted, their limitations need emphasizing. Prolificacy tables, like life tables, may be thought of as picturing the situation which would exist in a population cohort exposed throughout its life time to conditions of the base period. The extent to which these conditions are affected by what has occurred prior to the base period is not important with most life tables, for death rates at a given age during a given year probably would not be much higher (or lower) if death rates at younger ages had been considerably lower (or higher) during prior years. Actuaries recognize, however, that a great epidemic may be followed by a period of unusually low death rates; for example, that a United States life table for 1921 may reflect not simply the conditions of that year, but also the modifying effect of the influenza epidemic of 1918-20. With prolificacy tables the effect of prior years is much more significant, the most obvious reason being that the order of birth for

infants born in a given year is dependent entirely on the number of births to the mother previously.[32]

In view of the large decline in birth rates during the 20 or 30 years preceding 1919-21 or 1929-31, it is certain that the prolificacy distributions shown in Table 3 and Chart 2 differ greatly from those of a hypothetical generation coming into child-bearing under conditions of either period, and living through the child-bearing ages under those conditions. Such a generation undoubtedly would have a much lower percentage of wives with several children and a much higher percentage with less than 5 children, but the extent to which the percentage with no children would be higher is difficult to evaluate. Most populationists believe that the chief cause of the decline in the birth rate has been an increase in the proportion of couples practicing contraception effectively. Some of these couples presumably desire to prevent births entirely; to the extent that this group increases in numbers and is successful, the first birth rate is permanently lowered and the proportion of childless couples in this type of prolificacy table is permanently raised. Other couples, however, merely wish to lengthen the interval between marriage and the first birth, and to control the number of subsequent children and their spacing. From a long time standpoint, while the annual additions to this group are increasing, the effect is to decrease the first birth rate and increase the proportion of childless women in prolificacy tables like these. But once the group becomes large and the annual additions begin to decline, the first birth rate tends to increase and the proportion of childless women to decrease, and eventually fluctuate around their original values unless other conditions change. It is possible that the high proportion of childless wives shown for 1929-31 in Table 3 and Chart 2 is due in part to an increase in the additions to the group practicing contraception effectively.[33]

Fluctuations in prolificacy distribution may arise from a variety of other causes, among which economic conditions and war are important. A severe depression may result in the postponement of marriage, and of first births by couples already married, on a sufficiently large scale to raise significantly the proportion of childless women in a prolificacy table for a base period weighted with such depression years. The oppo-

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[32] Partial exceptions occur occasionally in cases of multiple births, and of women confined more than once during the year in question.

[33] For a discussion of the importance of contraception and of its effect on prolificacy, see Pearl, Raymond, *op. cit.* pp, 225-233.

site situation may arise during an ensuing period of prosperity, when the postponed first births occur in addition to those normally expected. Similarly the beginning and ending of a war may lead to a sufficiently large number of "war babies" and postwar babies as to exaggerate the first birth rate and minimize the proportion of childless women in a prolificacy table for such a period. It is possible that this factor, coupled with the wartime boom, is responsible for the abnormally low proportion of childless women in prolificacy tables for 1919-21, 1918-22, and 1917-23 (see Tables 2 and 3 and Chart 2).

Summing up, it is clear that the method of computing prolificacy distribution from census and vital statistics is technically sound, and should be of great use. It is equally clear that the shortcomings of the basic data now available for the United States lead to an appreciable margin of error in the results of the computations, and that the influence of conditions prior to the base period complicates greatly the problem of interpreting the results.

STUDIES ON THE BIOLOGICAL FACTORS IN PUBLIC HEALTH

1. THE TREND OF THE AGE OF MARRIAGE IN WASHINGTON COUNTY, MARYLAND, FROM 1897 TO 1938*

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INTRODUCTION

THAT individuals differ in their reactions to pathogenic agents and processes is among the oldest and best established facts about health and disease. Such differences are the expressions of variability, which, as Pearl (1939) notes, is to be included among the broad general forces or principles "upon which the doings of all living things are based." Recognition of the importance of variability in relation to problems of health has led to numerous studies of some of the factors associated with it such as age, sex, physique, race, nationality, occupation, marital, social and economic status, and so on. A comprehensive view of these and other characteristics point to the generalization that variability is the resultant of interaction between the hereditary constitution and the environment. Individual differences in liability, resistance and susceptibility to pathological aberrations begin with the formation of the zygote which bears already the imprint of those ancestral characters that are transmissible through the spermatozoa and ovum. They include not only the so-called hereditary diseases but such characters as physical structure and sex differ-

* Abstracts of the marriage certificates were obtained through the courtesy of Mr. Edward Oswald, Clerk of the Court for Washington County, Md. The tedious work of abstracting the records was done by the Misses J. Barner and A. Greene, Mrs. M. Trite and Miss L. Rohrer. The last named also assisted in the tabulation and computation of the data. The author takes this occasion to express his appreciation of their cooperation, and particularly that of Dr. W. R. Cameron, Health Officer for Washington County.

entials that directly or indirectly influence the chances of disease or death. During uterine life variability results further from the many influences deriving from the conditions of the maternal host and whatever circumstances may affect her, from accident and disease to inferior organization of the pelvic organs. In post-natal life, during infancy and early childhood the individual is subjected particularly to the effects of a unique social environment formed by his parents and siblings. These effects, it should be noted, result also to a great extent from the general hygienic condition prevalent in the home and therefore depend upon the degree of education, the kind of occupation and the general economic status of the family group. After infancy, the social environment of the individual is gradually expanded to include a greater portion of the other members of the group in which he participates. Throughout the years that follow, variability in relation to health and disease is further affected by the physiological changes associated with age and the cumulative results of the previous conditions of health.

When the elements of the individual variability are considered in these terms, outstanding appears the influence of the family unit as its source. For any individual, the parental group is both the fountain-head of the hereditary characteristics he possesses as well as his most important and effective environment, particularly in the first years of life. In these years, as has been said, the qualities of the family group determine among other things the kind of care received by the individual. But it must be realized that also the acquired traits associated with the health of the later years—education, occupation, social habits and beliefs—are also consequences of early family conditioning.

From these considerations it becomes apparent that for a comprehensive study of variability in relation to the health of a population, attention must be directed towards an adequate measure of the influence of the family group and consequently towards the development of an appropriate methodological approach to such an evaluation. Epidemiologists and geneticists particularly have for some time recognized the need to study the family unit in relation to health and disease. The former have had as an objective to arrive at measures of controlling the spread of contagion within the family unit, the latter have in the main sought to individualize the mechanism of the hereditary transmission of certain diseases. From both of these types of investigations a great deal of useful knowledge has been acquired, but their respective scope being in a sense limited they do not furnish the means of adequately studying the elements of variability. In particular, they fail to

consider or are unable to consider, the import of the social conditioning brought about by the family aggregation.

Recognition of the relevance of the family unit as an object of special study in relation to morbidity and mortality demonstrates that for a clear understanding of the factors affecting the health of a population due weight must be given to all the elements of population dynamics, specifically to those centering around the family group. The most pertinent concern the formation of the family group and its fertility, both manifestations of the fundamental biological urge to reproduce. In our civilization it can be said that on the whole the formation of the family together with its fertility is dependent upon the social institution of marriage (cf. Ciocco, 1938, 1939) and because of this the marital partners constitute the nucleus of any family unit. The qualities possessed by each of the marital partners not only can affect the other, but, of course, are reflected in the health of the offspring. Among these qualities, age is consequential because from the duration of marriage as well as from the age at marriage depend directly the chances of reproduction and indirectly to some degree the chances of survivorship of the embryo in utero and of the infant during the immediate post-natal life (cf. Yerushalmy, 1938). Age of marriage in relation to the general problem of fertility bears particularly on the question of the contemporary decline in birth-rate, since it is generally conceded that one of the manifestations of social and economic changes of the last century has been the postponement of marriage. Some have suspected (cf. Marshall, 1929; also Charles, 1937) that this has contributed to the observed fall in natality. In view of this and because the initial step in the present investigation on group health is concerned with a study of the family unit, it has seemed pertinent to inquire, first of all whether during the last forty years there has been a change in the marriage age of the population examined and how such change may be related to social and economic factors. The presentation of the results of this first study constitutes the scope of this paper.

SOURCES OF MATERIAL

This investigation will in its main aspects be concerned with the population of Washington County, Maryland, and particularly of its county seat Hagerstown. The choice of this locality as the place to pursue the present investigation is due to several reasons, the most important being a consequence of the kind and amount of data that

are available. These data consist of the records of local public and semi-public organizations and the records of special studies made from time to time by the U. S. Public Health Service. The first type of records has been obtained through the generous cooperation of the county, city and state health authorities, and those of other county and city departments, school authorities, and the directors of other organizations of a quasi-public nature. Thus the certificates of birth, death, marriage and divorce, data from the communicable disease lists, clinical reports of county nurses, have all been furnished for study.

The records of the special studies made by the U. S. Public Health Service date back to 1921. At that time the Office of Statistical Investigations initiated a study on morbidity in Hagerstown because certain facilities were already afforded by the location there of a health demonstration center and "partly because it was a fairly typical small city in that part of the eastern section of the country which had not been influenced greatly by recent immigration" (Sydenstricker, 1926). Together with a study of the morbidity in the general population, another study also on morbidity but having certain characteristics of its own was organized in the schools of the community (Collins, 1924). Records from other field studies are also at hand for analysis. These include the results of clinical examinations on samples of children and adults; measurements of hearing acuity (Sterling and Bell, 1930); observations on physical growth (Palmer, 1933); studies on vitamin A deficiency (Palmer and Blumberg, 1937); investigations on dental caries (Klein, Palmer and Knutson, 1938); roentgenographic examinations of the chest and tuberculin tests (McKneely, 1938); and functional examinations of the heart by means of stethograms and electrocardiograms (Boone and Ciocco, 1939). The mass of material thus collected during this period of time on large groups of children and adults signifies that a good deal of precise information is known about various aspects of the health of a considerable proportion of the population. In view of this, the need has been felt for a systematic organization and integration of the data at hand so that the information derived from these various sources can serve first of all as a background for a rational approach to the initiation of new studies; and secondly will give the opportunity to investigate further the means of synthesizing the many manifestations of health and disease. The methods followed in the organization and integration of the data will be described in detail in another paper.

Here it is sufficient to mention that one of the main aspects of this work is to bring together the information obtained on the members of

families of the same generation as well as of successive generations. This aspect of the work of organization will therefore furnish the necessary material for the achievement of the objectives of the investigation discussed here.

POPULATION CHARACTERISTICS OF WASHINGTON COUNTY AND HAGERSTOWN, MD.

Since this investigation deals primarily with the population of Washington County, Md. and its county seat, it is appropriate to describe briefly in this introductory paper some of the main statistical features of the population of the locality. Hagerstown and Washington County were first settled in the early part of the 18th century by one of the initial waves of westward migration which originated from the eastern seaboard of the American colonies. The fertility of the land combined with its natural beauties must have proven very attractive, because by 1790 the census records a population just under 16,000. Of this number, 8 percent were registered as colored. From 1790 the population has increased fourfold, being, according to the census, equal to almost 66,000 in 1930. In comparison, during this interval the population of the state of Maryland had increased about 5 times and that of the United States as a whole about 40 times. As expected, the growth of the population is proceeding at a decelerating rate. Between 1920 and 1930 the population increase equalled 10 percent, while between 1910 and 1920 it was 20 percent.

Native white persons of native parentage constitute a high proportion—93 percent—of the population. In all of Maryland this percentage equals 64 and in the country as a whole, 57. Foreign-born whites constitute only 0.9 percent and Negroes 3 percent of the population; while in Maryland the percentages are, respectively, 6 and 17; and in the United States, 11 and 10.

The majority of the population of Washington County lives in rural areas—53 percent were thus listed in 1930. The comparative percentages for Maryland and the United States are 40 and 44, respectively.

In 1930, the population of Hagerstown numbered almost 31,000 and therefore comprised 47 percent of the population of the county. Instead, in 1870 only 14 percent of this population was concentrated in the city which numbered 6,000 inhabitants. Hagerstown besides being the county seat is also the main trading center of the surrounding agricultural area and in addition contains a few small but important industries and the

shops of a railroad. The most rapid phase of the growth of the city took place between 1910 and 1920 when it almost doubled in size, rising from 16,000 to 28,000 inhabitants.

Notwithstanding the special characteristics of the composition of the population relative to race (color) and nationality, the trends of the birth and death rates of Washington County apparently have followed those of Maryland and of the United States. The birth-rate has decreased from 27 per 1000 in 1920 to 20 per 1000 in 1930. The comparative rates for Maryland are 25 and 19, and for the United States they are 24 and 19. In Washington County the mortality rate has declined from 14 per 1000 in 1915 to 12 per 1000 in 1930. In Maryland the drop has been from 16 to 13 per 1000 during this period, and in the United States, from 14 to 11 per 1000.

Summing up then, the population of Washington County has the following features:

1. A markedly high proportion of white persons and especially of native whites of native parentage.

2. A comparatively slow growth with an increasing and rapid concentration in the direction of the county seat.

3. A decline in natality and mortality of the same order as that noted for Maryland and the United States.

MEDIAN AGE OF FIRST MARRIAGES OF WHITE PERSONS, 1897-1938

During the 42 years from 1897 to 1938 there were registered in Washington County, Maryland, 15,078 marriages of white persons who where residents* of the county. In 2,018 marriages, 13.4 percent, one or both of the partners had been previously married and so was either widowed or divorced. In this inquiry these will be omitted so as to avoid additional complications introduced by the factors determining remarriage. Therefore, the results presented here will concern a total of 13,060 marriages or an average of 310 per year.

The frequency distribution of marriages according to age in any particular calendar year has characteristics of skewness generally familiar to all students of population statistics (cf. Yule and Kendall, 1937) and the data of this sample are no exception. Since there is a pro-

* These include the marriages in which (a) both the bride and groom stated that their residence was in Washington County, and (b) only the groom stated that he was a resident of the county. The choice of 1897 as a starting point for this study is associated with the fact that the registration of births first began in 1898.

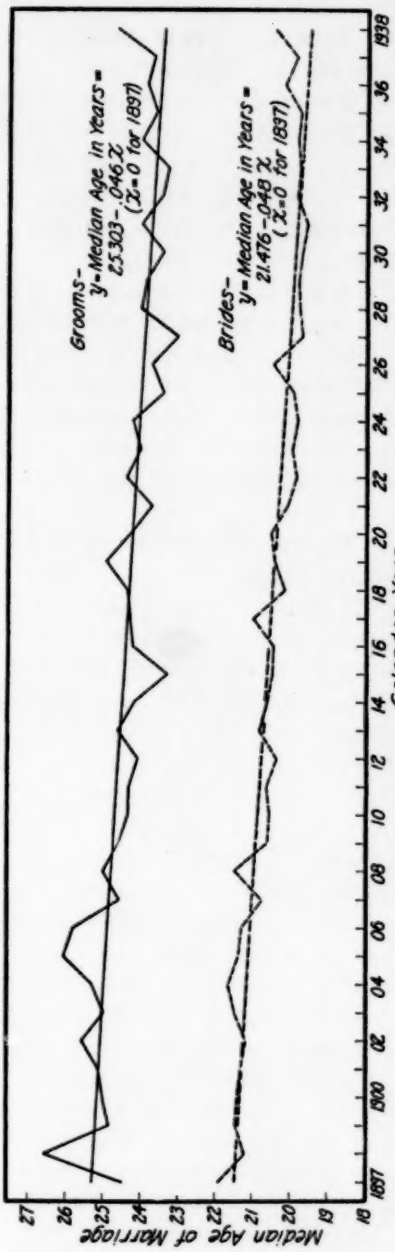

FIG. 1. MEDIAN AGE OF MARRIAGE OF WHITE MEN AND WOMEN IN WASHINGTON COUNTY, MD., FROM 1897 TO 1938
(Included are only first marriages of residents of the county.)

nounced concentration of the marriages toward the younger years the median is undoubtedly the more appropriate centering constant and will be used exclusively in this report. The median ages of the white men and women residents of Washington County and married there, from 1897 to 1938, are shown for each year graphically in Figure 1. For the grooms the median age ranges from a high value of 26.6 years observed in 1938 to a low of 23.0 years noted in 1927. For the brides, instead, the highest median age is equal to 21.9 years for 1897, and the lowest equals 19.6 years observed in 1931.

The median ages shown in Figure 1 seem to be in good agreement with the ages reported for certain particular periods or years in other parts of this country. Thus, from Notestein's (1931) tabulation of a sample of the census data for 1910 it is observed that the *modal* age for urban women was 20.5 years and for rural women it was 19.2 years. Similarly in New York state for 1921, the median age of the single grooms equalled 24.3 years, while that of the single brides equalled 20.9 years.

The results shown in Figure 1 are surprising and somewhat unexpected. First, it is seen that during the period under observation there has been no increase in the average age of marriage of either the bride or groom. Not only is there no evidence of an increase in average age of marriage but it would seem in fact that during these 42 years there has been a decline. The above figures on the highest and lowest medians would point to that, but more definitely is this trend demonstrated when to the annual medians a straight line is fitted by the least squares method. The resulting equation has a negative slope constant of almost the same value for both sexes and indicates a decline of the median age of marriage by about 5 percent or 18 days per annum. From Figure 1 it is also obvious that this apparent decline took place to a greater degree before 1915. In fact, if a separation of the data is made at 1915 and the trends calculated for the parts this impression is confirmed. In the case of the grooms the slope constant for the years 1897 to 1914 is equal to —.058, while for the period 1915-38 it has a value of only —.007, or less than one-eighth of the preceding. Similarly for the brides, from 1897 to 1914 the value of the slope constant is —.065, while from 1915 to 1938 it is reduced by about one-half to —.028. These findings show then that for both the brides and the grooms the greatest decline in the average age of marriage took place between 1897 and 1914 and that since 1914, particularly in the case of the grooms, the median age has remained almost constant.

These findings are rather unexpected. *A priori* consideration would lead one to believe, as some do, that the social changes that have occurred since the turn of the century would bring about a postponement of marriage because the economic needs of the family are greater now than in the past and because the chances of acquiring the means of satisfying these greater needs require today more time. However, the fact remains that the age of marriage has not increased in this population although its well-being has increased as is evident from the decline in mortality and natality—two of the most sensitive indices of material progress (cf. Niceforo, 1921). In order to understand better the significance of these findings the ages of marriage in relation to the socio-economic status of the groom at the time of his marriage will next be studied. In the meantime it seems safe to conclude that so far as this population is concerned the decline in birth-rate cannot be attributed to postponement of marriage since none occurred.

Before discussing in more detail the above findings it is pertinent to examine the range of the distribution of the marriage ages for each year, since this range is extensive. This is evident from Table 1 where are listed for each calendar year the value of the third quartile of the distribution, the age of the youngest and oldest groom and bride, and the difference between the highest and lowest ages. It is seen that the range of the age of marriage for any particular calendar year varies from 22 to 56 years for the grooms and from 21 to 56 years for the brides. The variation in range is due primarily to the annual differences in the ages of the oldest persons married. In 1933, for example, the oldest groom was 44 years old but in 1908 a bachelor aged 80 years of age was married. In the case of the brides, in 1904 the oldest was only 35 years old but in 1931 the oldest was a spinster 71 years of age. In both of these exceptional instances of senile nuptials an attempted verification appears to substantiate the declaration made on the marriage certificate. There is very little change from year to year in the ages of the youngest to marry. A boy of 15 married in 1915 and was the youngest to be recorded in any year, while in 6 instances the youngest brides were only 14 years of age.

Of greater importance are the figures on the values of the third-quartile ages of the distributions. For the grooms the values of the third-quartile vary from 24.6 to 28.3 years and for the brides, from 21.7 to 24.7 years. Consequently for the two sexes respectively, in any year, 75 percent of the marriages are accumulated below these ages. A comparison of these data in Table 1 with the corresponding medians pre-

TABLE 1

Third-quartile of the distribution of marriage age, age of youngest and oldest bride and groom and range of marriage ages for each calendar year from 1897 to 1938. Marriages of white residents of Washington County, Maryland

YEAR	AGE OF GROOM				AGE OF BRIDE			
	THIRD-QUARTILE	Lowest	Highest	Range	THIRD-QUARTILE	Lowest	Highest	Range
1897	26.8	18	52	34	23.5	15	36	21
1898	27.4	18	54	36	23.8	15	40	25
1899	27.6	17	52	35	24.1	16	49	33
1900	27.2	18	53	35	24.0	15	45	30
1901	27.3	18	61	43	24.3	16	36	20
1902	28.3	18	58	40	24.7	15	47	32
1903	27.2	18	67	49	24.3	16	47	31
1904	27.2	17	65	48	24.3	15	35	20
1905	28.0	17	61	44	24.3	15	50	35
1906	27.6	17	50	33	23.9	15	46	31
1907	26.5	17	54	37	23.5	14	40	26
1908	27.2	17	80	53	23.9	15	40	25
1909	25.6	18	40	22	23.0	16	38	22
1910	26.6	17	60	43	23.0	15	39	24
1911	26.4	18	50	32	23.4	15	39	24
1912	25.7	16	52	36	22.9	14	40	26
1913	26.4	16	60	44	23.3	16	60	44
1914	25.9	17	58	41	23.3	15	48	33
1915	25.2	17	54	39	22.7	15	47	32
1916	26.2	17	54	37	23.3	14	56	32
1917	26.5	16	49	43	23.6	15	50	35
1918	26.5	17	57	40	22.7	15	51	36
1919	26.1	16	68	52	23.5	15	66	51
1920	26.7	16	72	56	22.2	15	71	56
1921	26.6	16	47	31	22.5	15	54	39
1922	26.5	17	45	28	22.8	15	50	35
1923	25.5	16	50	34	22.5	15	52	37
1924	26.8	17	55	38	22.2	15	46	31
1925	25.1	17	57	40	22.2	14	45	31
1926	25.4	17	51	34	23.4	15	47	32
1927	24.6	16	45	39	21.9	15	52	37
1928	26.6	17	68	51	22.5	15	63	48
1929	25.4	17	60	43	22.2	14	56	42
1930	25.3	16	62	46	22.5	15	43	28
1931	25.4	17	62	45	21.7	14	47	33
1932	25.4	18	45	27	22.0	15	46	31
1933	25.3	17	44	27	22.2	16	39	23
1934	25.9	17	63	46	22.4	15	52	37
1935	25.8	18	51	33	22.6	15	44	29
1936	26.4	17	73	56	22.2	15	63	48
1937	25.6	17	47	30	22.8	16	40	32
1938	26.6	17	55	28	23.4	15	45	30

sented in Figure 1 shows that on the whole for both the grooms and the brides the third-quartile age is greater than the median by less than 3 years. Moreover, when the trends are estimated as above it is found that relative to the third quartiles the slope constants are also negative and of practically identical values as those of the medians. For the grooms and brides, respectively, the equations of the trend of the third-quartile age are:

$$27.451-.047\ x \text{ years } (x = 0 \text{ in } 1897)$$
$$24.073-.049\ x \text{ years } (x = 0 \text{ in } 1897)$$

From these findings it appears then that the decline in the average age of marriage from 1897 to 1938 has resulted from a movement of the whole mass of the distribution towards the younger ages.

OCCUPATION OF GROOM AND MARRIAGE AGE OF GROOM AND BRIDE

Data on the occupation of the groom are available only for the marriages that took place from 1915 on. But even since that period the information on this point has been found missing in approximately 25 percent of the marriage certificates. In classifying the recorded occupations, Pearl's (1933) method has been followed because it leads to a final segregation of the occupations into broad categories of different social and economical levels. The three general classes of occupation, according to Pearl's scheme, are: I. Owners, managers, officials and professional men; II. Skilled and semi-professional workers; III. Laborers —unskilled and semi-skilled. With the information given on the marriage certificate no difficulty was encountered in classifying the occupations in the specific groups except when "farmer" was recorded. Since under this heading are probably included farm laborers as well as farm owners it has seemed wiser to consider them separately and add a new class, namely "farmers," to the three mentioned above. In this class, it should be noted, fall 20 percent of the grooms whose occupation is given on the certificates. Omitting for the time being the "farmers," it is found that 9 percent of the remaining grooms whose occupations are stated belong to group I, 56 percent to group II and 35 percent to group III. These percentages may be compared to those obtained by Pearl (1939) from the United States Census of 1930. When farm owners, tenants and laborers are excluded, his computations show that in that period 17 percent of the gainfully occupied males above 10 years of age belonged in group I, 51 percent in group II and 32 percent in

group III. Thus, on the basis of this comparison there is a relative deficiency of persons of group I among the grooms of this population. But this is as it should be since the kinds of occupations classed in group I include some that are acquired with advancing age by promotion, as it were, from groups II and III.

The median ages of the grooms of the three occupational groups computed for the years 1915-1938 are presented in Figure 2.

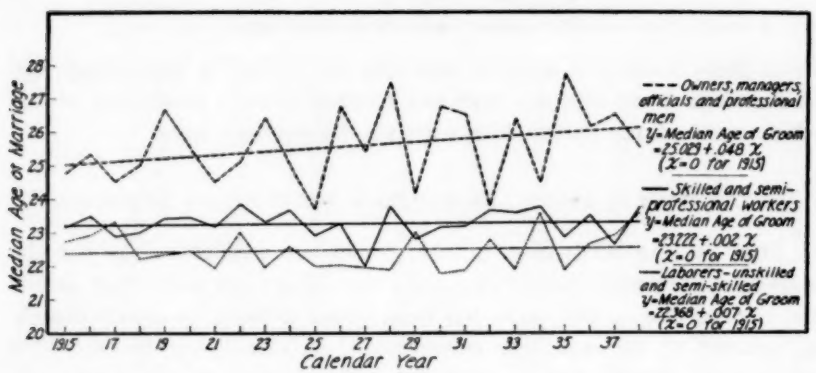

FIG. 2. MEDIAN AGE OF MARRIAGE OF WHITE MEN OF DIFFERENT OCCUPATIONAL GROUPS IN WASHINGTON COUNTY, MD., FROM 1915 TO 1938
(Included are only first marriages of residents of the county.)

The immediately outstanding feature of the data given in Figure 2 is the net difference in the marriage ages of the three groups of men. It should be realized that for any particular year the number of persons included in each occupational group is not large. On the average for each year group I contains 17 grooms, group II includes 111 grooms, while 70 grooms are contained in group III. So, while in any particular year the differences between groups are not always statistically significant, the fact that the differences are consistently present year after year and in the same direction, with only three minor exceptions, is rather impressive and indicative of the real importance of the social and economic factor with regard to marriage age. From these data it would seem that the process of acquiring a skilled trade has as a total effect on the age of marraige that of delaying it by one year, on the average; while the acquisition of a profession or a directive position of sorts, retards marriage by almost 3 years.

From 1915 to 1938 the trend of the changes in the marriage ages of the grooms of groups II and III has altered little or none at all. On the other hand, it would seem that there has been an increase in the age of marriage of the grooms of group I. For these the average age has augmented at the rate of about 5 percent annually. It may be that this is a real increase since for the occupations included in this class the necessary requirements have grown in recent years more difficult and time consuming. The group "farmers" (cf. Fig. 3) is the only one of the occupational classes of the grooms to demonstrate some tendency towards a younger median marriage age since 1915. But, as is known, this is a heterogeneous group, so for the time being the significance of this finding cannot be determined.

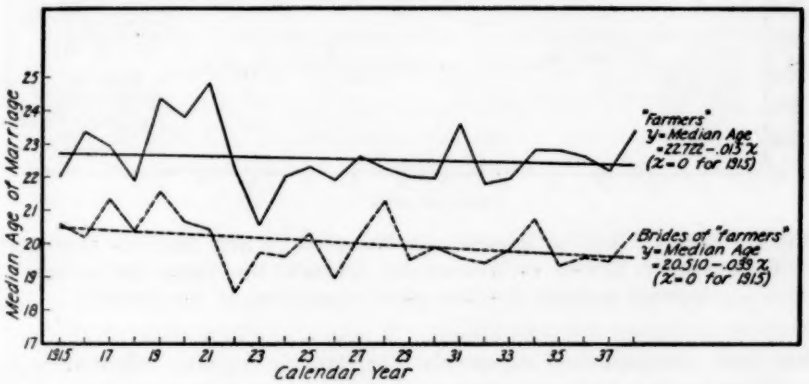

FIG. 3. MEDIAN AGE OF MARRIAGE OF WHITE "FARMERS" AND THEIR BRIDES IN WASHINGTON COUNTY, MD., FROM 1915 TO 1938
(Included are only first marriages of residents of the county.)

From Figure 3 it is also to be noted that the median age of the brides of "farmers" has declined at the rate of about 4 percent annually since 1915. With respect to age they occupy a place between the brides of the grooms of group II and those of group III as is apparent by comparing Figure 3 with Figure 4.

This graph brings out in the same striking and decisive manner as Figure 2 the effects of the social and economic status on the age of marriage. Each year, the brides whose husbands belong to group III marry at the youngest median age while those whose husbands are in group I marry at the oldest ages. In Figures 2, 3, and 4 are exhibited

then the manifestations of two phenomena. The first is the influence exerted by the social factor on the age of marriage of the male. It appears that social or other pressure (cf. Ciocco, 1939) obliges the man to wait before marriage until the designed objective in terms of occupation or profession is reached.

The second phenomenon manifested in these data is the high correlation between the marriage age of the bride and groom, a fact which

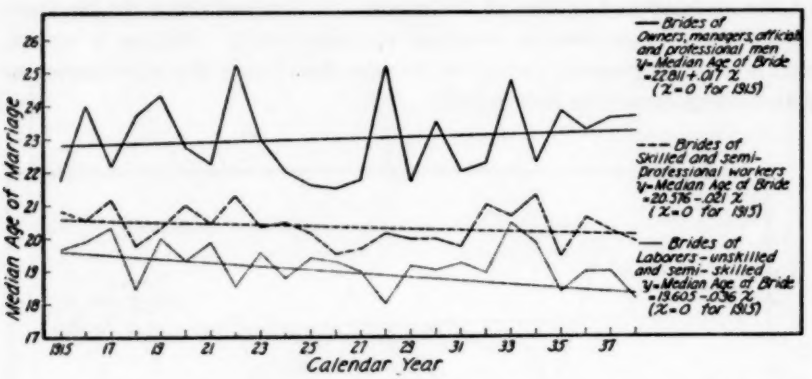
Median Age of Marriage
Calendar Year
— Brides of Owners, managers, officials and professional men
y = Median Age of Bride
= 22.811 + .017 x
(x = 0 for 1915)
--- Brides of Skilled and semi-professional workers
y = Median Age of Bride
= 20.576 - .021 x
(x = 0 for 1915)
— Brides of Laborers - unskilled and semi-skilled
y = Median Age of Bride
= 19.605 - .036 x
(x = 0 for 1915)

FIG. 4. MEDIAN AGE OF MARRIAGE OF BRIDES OF WHITE MEN OF DIFFERENT OCCUPATIONAL GROUPS IN WASHINGTON COUNTY, MD., FROM 1915 TO 1938
(Included are only first marriages of residents of the county.)

has been demonstrated repeatedly (cf. Harris, 1930). Here it is in evidence with relation to the occupational class differences in age of marriage and also in some degree relative to the trend of the median ages in each occupational class from 1915 to 1938. That the brides demonstrate the same behavior as the grooms with regard to marriage age and occupational class cannot be all attributed to the greater time needed for the women to acquire an education or a technique corresponding to the higher level of the husband. While this might be true for the persons of Class I there certainly is no evidence to suppose that more training other than that which a woman can acquire before 18 years of age is necessary in order that she is acceptable to marry an artisan instead of an unskilled laborer. The correlation between the ages of bride and groom is exhibited also in the comparative trends of the median age of marriage from 1915 to 1938. As it has been remarked, after 1915 the brides still show a slight tendency towards younger marriage while

the median age of the grooms has remained constant for all practical purposes. The difference between the male and female trends is reflected in each occupational group. It is seen that the brides of the "farmers" (who are the only grooms to exhibit a tendency towards a younger marriage age) demonstrate the strongest tendency towards younger marriage than all the other groups of brides. On the other hand the brides of owners, professionals, etc. (whose median age shows an increase from 1915 to 1938) also show a tendency in the same direction but very slight. From the comparison of the trends for males and females of each occupational group the results could be interpreted to mean that two contrasting forces are acting on the age of marriage of the women; one is the association factor that draws the females to follow the trend of the males, the second is some other factor that is causing a progressive decline in their marriage age.

In conclusion, from this analysis the following facts seem pertinent:

1. There is a definite association between late marriage and socio-economic level observed in every year from 1915 to 1938. Men who are owners, officials, professionals, etc. marry later than the artisans who in turn marry later than unskilled laborers. The brides exhibit the same differences as do the grooms.

2. During the years from 1915 to 1924 little change has been noted in the median age of marriage of the males, taking all occupational classes together. This is due to the stability of the average marriage ages of the two largest groups, the skilled and the unskilled workers. On the other hand, contrasting trends are to be noted for the small group of owners, professionals, etc. whose marriage age seems to increase, and the "farmers" whose average age exhibits a slight tendency to decrease.

3. During the same period the women on the whole have shown a very slight tendency towards younger marriages. This is to be noted for all the socio-economic groups except the one containing the brides of men who are owners, professionals, etc.

With regard to the general trend of marriages from 1897 to date, these facts indicate that the decline noted in the males from 1897 to 1915, if real, may have been due either to a shift in the composition of the population according to occupational classes or else to some other factor or factors, of social or biological nature such as that exerting its influence on the age of marriage of women and of the "farmers." In all events an increasing age of marriage is apparently to be noted only

for the numerically small group of owners, managers and professionals. If the facts observed here have general application it would mean that a change towards higher ages of marriage is not to be expected in the general population so long as the conditions remain the same as those present at least since 1915. Whether or not the women really tend to marry younger now than in the past deserves a more careful and detailed study than the information at hand permits. Because of the general importance of this fact, if real, an effort should be made to discover its causative elements.

The observed socio-economic differential related to the age of marriage is in agreement with the findings of Kiser (1937) and particularly of Notestein (1931). Notestein examined the census returns for 1910 and arrived at a *modal* marriage age of women equal to 23.5 years for the wives of professionals, 21.7 years for the wives of the proprietary, 21.3 years for the wives of clerks, 19.6 years for the wives of skilled workers, 19.5 years for the wives of semi-skilled workers and 18.5 years for the wives of unskilled workers. The observations reported here on the population of Washington County not only demonstrate to the same degree the effect of occupation on the median age of marriage of the groom as well as of the bride, but in addition show that this differential has persisted during the period from 1915 to 1938.

From the standpoint of the general problem investigated the most important effect of these socio-economic differences in age of marriage is related to the differences in the fertility, reproductive wastage, and infant mortality of the social classes. It is in relation to these consequences that further study on this problem will be conducted.

SUMMARY

This paper presents: (a) an outline of the general objectives of an inquiry into the fundamental factors related to the health of a population, the methodological approach and the sources of material; (b) a brief description of the population of Washington County, Maryland, where the investigation is being conducted; and (c) an analysis of the trend of the marriage ages in this locality from 1897 to 1938 with particular reference to occupational status.

From this analysis it has been found:

1. Since 1897 the median age of marriage has not increased but on the average has declined by almost 5 percent annually for both the men and women.

2. This decline has been particularly noticeable from 1897 to 1914. In fact, since 1915 for the males at least, the median age has remained constant.

3. Since 1915 the median ages of the men who were skilled and semi-professional workers and of those who were laborers at the time of their marriage have not altered appreciably. The median ages of owners, managers, officials, and professional men has increased on the average, while those of "farmers" has declined.

4. With regard to the brides, since 1915 the median ages of all except those marrying owners, managers, officials and professional men have slightly declined on the average.

5. Since 1915, the median age of marriage of the owners, managers, officials, and professional men has been consistently about 2 years higher than the average age of the skilled and semi-professional workers. In turn, these have averaged about one year older than the laborers. The same differences are noted relative to the average ages of the brides.

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MESSANCE: FOUNDER OF FRENCH DEMOGRAPHY

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OF THE eighteenth century French writers who made use of statistical materials when treating subjects in the field of demography, probably none was of more immediate significance, as a practical demographer,[1] than the collector of revenue in the Election de Saint-Étienne, M. Messance.[2] A lineal in-

[1] Antoine Deparcieux (1703-68), who concerned himself principally with the duration of human life and the construction of life tables, is generally considered to have been the first great French vital statistician. For an account of his work, see Michel Ptoukha, Proceedings of the Congrès international de la population, Vol. II, *Démographie historique,* Paris, 1938, pp. 79-91. Deparcieux's chief demographic work was his *Essai sur les probabilités de la durée de la vie humaine,* Paris, 1746. In 1760 a small "Addition" to this work was published.

[2] Nothing seems to be known of the details of the life of M. Messance. With the exception of Joseph Garnier's account (see *Dictionnaire de l'economie politique* (Paris, 1864), II, pp. 158-59, no reference to Messance appears in the dictionnairies, encyclopedias, etc., in which one expects to find accounts of the life and work of significant men. Messance's work has been attributed to both M. de la Michodière, intendant of the *généralité* de Rouen, whose secretary Messance presumably was, and to the Abbé Audra, professor of history at Narbonne. Messance declared himself "receveur des tailles de l'élection de Saint-Étienne" in his first work; and "receveur particulier des finances de Saint-Étienne en Forez" in his second work (see below, note, for titles of works). See note below; Garnier, *loc. cit.*; J. M. Querard, *La France littéraire,* Paris, 1834, VI, p. 89; also *La France littéraire;* Paris, 1769, I, p. 337, where Messance is called "Messange." Moheau (see note, below) referred to Messance as "Messenne." The first of Messance's works is referred to as "le travail de M. de la Michodière redigé et publié par M. de Messence," in the article "Population" in the *Encyclopédie Méthodique* (Économie politique et diplomatique, III), LXXIV, Paris, 1788, p. 669, and in J. Peuchet's *Dictionnaire de la geographie commercante,* I, Paris, 1801, p. ccccxlii. A careful search through the archive materials relating to Rouen and Saint-Étienne would probably clear up Messance's identity. Fortunately, in this essay, I am concerned with his ideas, not with his identity.

tellectual descendant of such French advocates of census taking as Bodin, Montchrètien, and the Abbé de Saint-Pierre, of such exponents of "political arithmetic" as Melon, and of such statistical compilers as Vauban, Messance was the first French writer to demonstrate at length the practical usefulness of birth and death returns. He was the first to employ such materials to refute the commonly held belief that the population of France was not growing. His work apparently was responsible for the fact that the intendants were later required to turn in reports on births and deaths. His discussion of demographic questions, moreover, illuminates the framework within which such questions were examined in eighteenth century France.

At the time of the appearance of his first work in 1766,[3] it was commonly believed either that the population of France had diminished, or that the population of France was not growing. The reports of the French intendants at the close of the seventeenth century, together with the picture of the wretched lot of the French peasantry, as set forth in the writings of Vauban, Boisguillebert, Fénelon, and La Bruyère, had produced in the minds of many Frenchmen the impression that the French economy was in a state of decay.[4] Montesquieu, who stated both in his *Lettres persanes* (1721) and in his *L'espirit des lois* (1748) that the modern world was less populated than the ancient,[5] had done much to create a climate of opinion favorable to the spread of the view that

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[3] *Recherches sur la population des généralités d'Auvergne, de Lyon, de Rouen, et de quelques provinces et villes du royaume, avec des réflexions sur la valeur de bled tant en France qu'en Angleterre depuis 1674 jusqu'en 1764,* Paris, 1766. Hereafter this work will be cited as *Recherches.* In the *Catalogue générale des livres imprimés de la Bibliothèque Nationale* two other short pieces are listed as by a "M. Messance": *Essai sur l'arithmétique, religieuse, morale, et politique,* Paris, 1782; *Hommage à l'assemblée nationale,* Paris, Lille, 1790. The last deals with a project for a perpetual tontine. Copies of these two works are not obtainable in the United States, so far as I have been able to discover.

[4] The origin of this belief is treated in my forthcoming *Population and Wage Theory in Eighteenth Century France;* see my *France Faces Depopulation,* Durham, 1938, pp. 102-107, and Hazel Van Dyke Roberts, *Boisquilbert. Economist of The Reign of Louis XIV,* New York, 1935, especially pp. 3-10.

[5] Already in 1685 Isaac Vossius (*Variarum Observationum Liber,* London, 1685) had said that parts of the ancient world were more densely populated than the modern. In a comment (1685) Peter Bayle assented to this view, but at the same time implied that Vossius greatly underestimated the population of France, as of that period. See *Oeuvres diverses de M^r Pierre Bayle,* The Hague, 1737, pp. 212-13.

numbers were not growing in France. Followers of Montesquieu, such as the Encyclopedist, D'Amilaville, agrarian precursors of the Physiocrats, such as Goudar and Herbert, with their assertions that France was much underpopulated, and Quesnay and his followers, with their contention that the French population had decreased, had helped to give currency to the latter belief, for most of these writers were in revolt against the political and economic absolutism then dominant in France, and found in the charge that numbers were not growing a convenient peg whereon to hang the demand for social reform. In fact, according to Messance, following the appearance of Mirabeau's *L'ami des hommes* in 1756, "nearly everybody upon the word of the author, believed in the depopulation of France."[6] Moreover, it was to the desire of M. de la Michodière, then intendant of Auvergne, to verify or refute Mirabeau's charge of depopulation that the conception of *Recherches* apparently is to be traced.[7]

I

Having described *Recherches* as a work devoted to facts rather than to interpretations,[8] Messance devoted the bulk of his study to the presentation of statistical data on births, deaths, and marriages in the *généralités* surveyed. His principal conclusions were two in number. First,

[6] *Nouvelles recherches sur la population de la France avec des remarques importants sur divers objets d'administration*, Lyon, 1788, p. 4; also *Recherches*, preface. Messance's statement was corroborated by Baron Frederick Grimm who wrote (October, 1766) that Messance's assertion that the population of France had increased since 1700 was contrary to "all remonstrances that all the parlements have made to the king for fifteen years, to all the ideas spread through all the political writings which have appeared in the same space of time, and to opinion generally received among enlightened men and among the people." See Grimm, *Correspondance*, Paris, 1813, Part I, Vol. V, p. 316.

[7] Mirabeau's charge led De la Michodière to order the compiling of births, deaths, and marriages in Auvergne for 1690-99 and 1747-56, for the purpose of comparing them and ascertaining whether or not the population actually had declined. Later he had the returns of Lyon and Rouen examined when serving as intendant in these districts. In 1759 Messance, then secretary to De la Michodière, was charged to continue and complete the compilation and analysis of the data. Having completed the study upon the eve of his appointment as *Receveur des Tailles de l'Élection de Saint-Étienne*, Messance was requested by De la Michodière to publish the work under his (Messance's) name. For this account of the genesis of *Recherches* see *Nouvelles recherches*, pp. 4-5.

[8] See *Recherches*, preface.

inasmuch as the average annual absolute number of births and marriages was appreciably greater around 1750 than around the close of the seventeenth century, it followed that the population of the *généralités* of Auvergne and Lyon had increased since 1700, not that it had decreased as the depopulationists had contended.[9] Second, since the average number of births per marriage was greater in the later than in the earlier period, it was to be inferred that marriages were more fertile, that debauchery and libertinage were not more common than formerly, and that "Nature has lost none of her claims (droits)."[10]

Messance's statistically supported conclusion that France's population had increased since 1700, and was increasing, served, probably more than any other work, to dissipate the belief that France's population had not grown and was not growing; it supplied a factual foundation to the inference of writers, such as Voltaire, that since economic conditions had improved, numbers must have grown.[11] "As day chases night," so, according to Messance, did his simply presented and well arranged facts "dissipate errors and fix opinion." No longer did one hear the "anti-patriotic cry" of the 1750's and early 1760's: "France is depeopled, she lies fallow, she is on the brink of ruin."[12] The French government, impressed with the usefulness of vital statistical data, as employed by Messance, after August, 1772, required the French intendants to recapitulate by *election and généralité* the number of births, deaths, and marriages,[13] the reports to begin with 1770.

Messance reached a number of other conclusions, as a result of his analysis of the statistical data compiled in *Recherches*. Families of six or more living children comprised not over one-twenty-fifth of all families, and families with 10-12 children comprised not over one-

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[9] *Recherches*, pp. 21, 56-58; also preface. Only in Rouen had the population apparently diminished (*ibid.*, pp. 99-101).

[10] *Ibid.*, pp. 21-22.

[11] See Spengler, *France Faces Depopulation*, pp. 106-107.

[12] *Nouvelles recherches*, pp. 1-2.

[13] *Ibid.*, p. 5. Moheau, the most important of the eighteenth century French writers on population, obtained from Messance (whom he refers to as M. Messenne) "mes premières idées" and a "grand nombre de materiaux." Moheau's praise of Messance's work does not suggest that the latter was an intendant. See Moheau, *Recherches et considérations sur la population de la France*, Paris, 1778, I, pp. xi-xii. On Moheau, see my "Moheau: Prophet of Depopulation," *Journal of Political Economy*, 47, 1939, pp. 678-691. On the history of demographic statistics in France, see John Koren (ed.), *The History of Statistics*, New York, pp. 237-66.

thousandth of all families.[14] Family size varied by place as well as in time, families tending to be more "fecund" in Lyon than in Auvergne, and in Auvergne than in Rouen.[15] Mortality was high, four-ninths dying by the fifth year, and two-thirds by the thirtieth year, in Lyon and Rouen.[16] The relative frequency of births and deaths fluctuated from month to month.[17] Messance drew attention to the fact that the Savoyards came to France to earn a living, only to return home years later, and that many Auvergnats migrated elsewhere to work, later to return home; but he presented no statistical picture of this migration.[18]

Messance limited himself to a small number of general observations relative to the factors influencing population growth. Celibacy, whether ecclesiastical or traceable to such employments as urban and rural domestic service, was unfavorable to population growth, he said.[19] Apparently he did not consider celibacy an important check, however;

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[14] *Recherches,* pp. 144-146. Moheau's data indicate that 3.4 per cent of the families enumerated numbered 6 or more children. A smaller sample, given by Moheau, indicates that the percentage of families, including the following number of children respectively, were: none, 27.3; 1-2, 46.7; 3-5, 23.5; 6-10, 2.5. Computed from Moheau, *Recherches,* I, pp. 133-134. Colbert's law of 1666, designed to stimulate population growth, provided pensions for noble and bourgeois families of 10-12 or more children, living or killed in the king's service, and not in the service of the church. See L. Schöne, *Histoire de la population française,* Paris, 1893, pp. 139-55.

[15] *Recherches,* p. 146, also pp. 177-214. Messance often compared what he called the average number of children per marriage, as of different places and time periods. Generally he would divide the number of marriages for a given decade into the number of births in that decade and use the quotient as the index of fertility by marriage. See *ibid.,* pp. 177-214; also *Nouvelles Recherches,* pp. 14, 17, 22-23, 53. He was not aware apparently of the shortcomings of this measure. On these shortcomings, see R. R. Kuczynski, *The Measurement of Population Growth,* New York, 1936, pp. 33-64.

[16] *Recherches,* pp. 155, 167, 172.

[17] The months, arranged in order of relative frequency of births in 1724-63, ran as follows from high to low: March, January, February, April, May, August, October, September, July, November, December, June. The corresponding order for deaths was: March, April, May, January, February, December, June, July, October, September, November, August. See *Recherches,* pp. 179-80. As late as 1855-65 the arrangement of months by order of birth frequency remained similar to that given by Messance. See E. Levasseur, *La population française,* Paris, 1889-92, II, p. 21.

[18] *Recherches,* p. 5.

[19] *Ibid.,* p. 105. Many eighteenth century French writers referred to celibacy, whether lay or ecclesiastical, as an important check to population growth.

for he observed that men are "naturally" impelled to perpetuate themselves, and that the number of celibates had diminished.[20] He said that population growth varied directly with the number of marriages and their "fecundity," but did not question whether or not there might be an inverse relationship between fertility per marriage and frequency of marriages.[21] He seems to have supposed that numbers did not tend to become too great or too little.

> There is reason to believe that the destruction of the human species is proportioned to the means that it has of multiplying itself; if it were otherwise, the population would perish or multiply too much.[22]

In *Recherches* Messance subscribed to the view that population growth was not subject to legislative control.[23] There was no reason to believe that Colbert's measures had reduced the number of celibates or augmented the number of children.[24] Moreover, the factors govern-

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[20] *Ibid.*, p. 143.

[21] *Ibid.*, p. 105.

[22] *Ibid.*, p. 106. He did not comment relative to the advisability of further population growth. He did say that, since males aged 0-14 years both consumed more than females 0-14 and were relatively less numerous, it followed that the greater the ratio of males 0-14 years of age to females 0-14 years of age, the more flourishing was the population (*ibid.*, p. 105).

[23] Elsewhere (*Recherches*, pp. 21-22), he implies that marriages were more or less fertile in proportion as libertinism and debauchery were less or more widespread; but he recommended no legislative control of such behavior.

[24] Colbert's measures were no longer in effect, Messance stated (*ibid.*, p. 143). The provisions applying to bourgeois families had been revoked in 1683; those applying to the nobility, although never formally revoked, were regarded by the government as no longer in force. In an article, "Government Aid to Large Families in Normandy, 1764-86" (to appear in *Social Forces*), based upon archive materials, my colleague, S. T. McCloy, shows that French government officials often expressed interest in stimulating population growth; that heads of large families often cited Colbert's laws when appealing to the state for aid; that at least twenty-six heads of large families in the *généralité* of Rouen requested aid in 1764-86, most of them receiving small but temporary grants. Among the reasons given by the intendant at Rouen (apparently De la Michaudière) to the Paris government in support of refraining from making many money grants to applicants, was the statement that in Rouen there were many (one time he said "infinite number") families containing ten or more living children, most of whom would ask for aid were it made available. Messance's data indicate, however, that only 11 of 15,023 families included ten or more children (*Recherches*, p. 146). Moheau (*op. cit.*, I, p. 134) gives the same figure.

ing the frequency and the fecundity of marriage were not generally amenable to legislative control.

> In effect, marriage depends upon the will of men and upon their character; and their inclination in this regard will never be subordinated to the will of the Legislator: the fecundity of marriages depends upon causes absolutely independent of the will even of those who alone are able to contribute to it, and is for this reason above all human laws.[25]

In a discussion of the relation of grain prices to wages, included in *Recherches,*[26] it was said that mortality was less, and health was better, when grain was abundant and low in price; that money wages did not move directly with grain prices; that real wages tended to rise when grain prices fell; that real wages and the standard of living tended to rise when per worker output increased.[27] In short, numbers did not necessarily and inevitably tend to press upon subsistence.

II

In *Nouvelles recherches,* described in the approbation of the Royal Censor as favorable "to the alleviation of humanity" and as very useful to administrators, Messance included a much wider range of subject matter than had gone into *Recherches. Nouvelles recherches* includes a number of comments upon "the precious book" of Jacques Necker,[28] minister of finance (1776-81); discussions of certain French tax problems, and of demographic questions posed in the works of Necker and several of his contemporaries; and new statistical materials pertaining to the growth and distribution of the French population. The chief purpose of his new work, Messance said, was to facilitate comparison

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[25] *Recherches,* p. 143.

[26] Commenting upon Adam Smith's reference to these reflections upon the nature of the relationship between grain prices and wages, Messance later said (*Nouvelles recherches,* p. 3): "Je dois dire que ces Reflexions ne soit à moi."

[27] *Recherches,* pp. 280-83, 287-92. The French Physiocrats, against whom these arguments were directed, had contended that high grain prices were favorable to all groups in the French population; that an increase in grain prices tended to benefit rather than to injure the French working population. See G. Weulersse, *Le mouvement Physiocratique en France,* Paris, 1910, II, Book iv, chap. ii, Book v, chap. iv.

[28] *De l'administration de finances de la France,* Paris, 1784. This work, said Messance, had caused him to write *Nouvelles recherches;* it was responsible for such merit as the latter possessed. See *Nouvelles recherches,* pp. 2-3. But see next footnote.

between demographic conditions in the earlier and those in the later parts of the eighteenth century.[29] For the sake of convenience I shall treat first his general observations with respect to problems of population, and then his specific discussion of French problems.

With somewhat more qualification than in his earlier work, Messance stated that legislators could do little to alter the prevailing trends in population growth. He thus rejected by implication, as he had already done in his first work, the reasoning of those writers who believed that population growth could be greatly accelerated through the enactment and enforcement of appropriate legislation.[30] The "sole means by which Governments are able to influence their Population" consisted in allowing men and women of appropriate age to marry, in maintaining the purity of the *moeurs,* in preserving the health of the people, and in assuring "liberty of person and to each his property."[31]

At the same time he adhered to the thesis, enunciated in his earlier work, that depopulation was unlikely. Insofar as he was explicit in respect to the possibility of depopulation, he indicated that it could develop only if "a local vice" continued to operate,[32] or if the volume of

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[29] Messance wrote (*ibid.,* pp. 2-3) that while everyone knew "that this beautiful Kingdom has a great Population, a good Agriculture, Manufacture, and a flourishing Commerce," it was desirable, since great changes were being made in the nation's administration, to determine "if the new manner is preferable to the old, by comparing the situation then with that of today." Messance's reference here is not quite clear. His work was completed in 1787 at the latest, for the date of the approbation of the Royal Censor is given as October, 1787. The reign of Louis XVI began in 1774, at a time when the financial condition of the French government was precarious. Outgo exceeded income, in part because of governmental extravagance, in part because the wealthy privileged classes were exempt from taxation. As a consequence of the failure of Turgot, Necker, and Calonne to solve the problem by taxing the privileged classes, the Assembly of Notables was called to meet in January, 1787. Although nothing was accomplished it was made evident that the States-General would have to be called. Thus Messance's work was completed at a time when significant administrative changes seemed to be foreshadowed.

[30] For the views of some of these writers, see my *France Faces Depopulation,* chap. x, and my *Population and Wage Theory in Eighteenth Century France.*

[31] *Nouvelles recherches,* p. 24.

[32] *Ibid.,* p. 6. He did not explain what he meant by "a local vice." In view of what he said of "depravation" of the *moeurs* (see below) he did not refer thereto. He may have referred to sickliness of climate. In a description of Balaruc, where deaths were exceeding births, he noted that the average length of life was low and mortality great because "stagnant waters viciate the air." He observed, too, that

subsistence underwent a permanent diminution.[33] He supposed, in general, as in *Recherches*, that the forces destructive of population tended to be counterbalanced by factors favorable to population growth. He observed that natality was high in both warm countries and those parts of France where mortality was high.[34] He did not believe that "depravation of the *moeurs*" would eventually produced depopulation.[35] He admitted that war, bad harvests, dearth of necessities, unemployment, and epidemics checked population growth, yet implied that the effects of these checks were temporary.[36]

Messance seems to have believed that population tends to grow rapidly so long as employment and subsistence are easily attainable. An excess of births over deaths is proof that population grows and "consequently that subsistence and work augment."[37] In newly settled areas population grows rapidly.[38] In older and well-settled countries population grows slowly, and sometimes not at all; for there subsistence is no longer easily attainable.

> The population having nearly attained the level of subsistence that the earth is able to furnish it, from that time on it establishes a new order of things. When the number of men is in this measure, the population ceases to be increasing; it becomes stagnant, and the means of propagation balances itself with the means of destruction, nevertheless, with an excess of births over deaths, to repair the losses that wars and epidemic maladies occasion.[39]

Messance was not essentially a populationist. He looked with favor upon "the multiplication of men, . . . the increase of their happiness and the perfection of their Knowledge";[40] and with disfavor upon depopulation.[41] Yet at no time did he dwell upon the advantages of population growth, or advocate its stimulation. Presumably he believed that the problem of numbers would solve itself, given a good government and comparative laissez faire.

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the number of births per marriage was relatively low but offered no explanation other than to observe, in another connection, that the number of domestics was relatively great. See *ibid.*, pp. 13-21, 70.

[33] *Ibid.*, pp. 6-7.

[34] *Ibid.*, pp. 12, 20.

[35] *Ibid.*, p. 27.

[36] *Ibid.*, pp. 24-25.

[37] *Ibid.*, p. 25.

[38] *Ibid.*, p. 12.

[39] *Ibid.*, p. 6. He refers here to the losses suffered in years of war and epidemics.

[40] *Ibid.*, p. 26, also p. 2.

[41] *Ibid.*, pp. 2, 6.

III

In his treatment of French demographic conditions Messance discussed secular demographic trends, alleged checks to population growth, demographic metabolism, factors influencing the distribution of population, and governmental policy with respect to the coal supply. He did not condemn mercenary wet nursing and *rentes viagères* as inimical to population growth, as did many of his contemporaries; nor did he devote much attention to the relation between military policy and population policy.

He described as improbable the proposition that France had once been more densely populated, declared that the augmentation of births since 1700 signified population growth, and indicated that the future demographic growth of France would be conditioned largely by economic factors. In reply to those who asserted that France had been more thickly populated in the fifteenth than in the eighteenth century, he said that the churches dating from earlier periods were quite small; that no abandoned fields, houses, or cities existed to attest to a greater population in the past; that the tithes on new land implied the clearing of land formerly uncultivated.[42]

Although he inferred from the growth in the absolute number of births since about 1700, that the population of France had increased in the eighteenth century,[43] he noted that in parts of France the population had not grown in recent years,[44] and that in France as a whole the population had been nearly stagnant in 1771-80, births exceeding deaths on an average by only 120,000 per year,—i.e. by an amount merely sufficient to counterbalance the losses due to occasional wars or "great mortalities."[45] He implied that whether or not this excess would become

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[42] *Ibid.*, pp. 63-64. On p. 76 he states that the great wall surrounding Paris exists to facilitate the collection of duties on provisions entering Paris, adding that he makes this observation to save future savants from writing endless dissertations on why the wall existed.

[43] *Ibid.*, pp. 65-69, 77-79.

[44] He referred in particular to Marseilles and Toulon whose population, he estimated, had decreased in 1760-80. Let the doctors find the causes, he said, and inform the government. See *ibid.*, p. 77, also pp. 13-21.

[45] *Ibid.*, p. 6. Messance estimated the French birth rate at about 40 per 1000; the birth rates of French cities and Paris at perhaps slightly over 36-37 and 33 respectively; the French death rate at about 34 per 1000. The average observed actual rate in three *généralités* was slightly under 40. See *ibid.*, pp. 8-11, 53. He believed that one could obtain the population of France by multiplying by 25 the

great enough to insure continued and permanent population growth, depended upon whether or not the economic base of the nation expanded.

> If her agriculture, if her industry are perfected and extended, it will people itself more; if, on the contrary, her productions and her work diminish, the number of her inhabitants will diminish also.[46]

IV

Among the important checks to population growth in France, according to many of Messance's contemporaries, were the decay of the *moeurs*, the growth of luxury, urbanization, misery, and the heaviness of the military and tax burdens. Messance, however, attached importance only to the first of these factors, and added that not even this factor would bring about depopulation, as some writers averred. In his opinion "depravation of the *moeurs*" merely slowed down the rate of growth.

> All these errors[47] of the human spirit are to morality and the population, that which frost, great rains, great droughts, fogs, hail, winds and tempests are to the physique and to the productions of the earth. All these attacks of evil against the good, derange, turn aside, and are able to retard the execution of the Laws by which everything is governed, but not to destroy them nor to change them.
>
> Thus, so long as there will be men and women there will be children; just as there will be subsistence to nourish them, so long as there will be men to cultivate the earth: all the difference will consist in the more or the less: for the potential force being above all the obstacles, nothing will be able to prevent its arriving at its end, which is to conserve and to perpetuate through reproduction.[48]

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average annual number of births, for a ten-year period, ten years being necessary for the supra-average fluctuations in the annual number of births to equalize the infra-average fluctuations. He supposed the population to approximate 24 millions. See *ibid.*, pp. 6-9, 53.

[46] *Ibid.*, pp. 6-7.

[47] Among "these errors" he explicitly placed both "the calculation which impels man to celibacy" (other than religious celibacy), or "to want only one or two children," and "the false grandeur which induces man to have a great number of domestics, a great number of guests at his table, in place of finding himself surrounded by his children; and the greatest deprivation, that which crowns everything, to destroy in the act of sowing." See *ibid.*, p. 27. He did not refer to mercenary wet-nursing, however, or to the alleged failure of mothers to suckle their young.

[48] *Ibid.*, p. 27. Elsewhere (*ibid.*, pp. 22-23) he states that the *moeurs* were "better" in 1760 than in 1700, inasmuch as the ratio of births to marriages was greater about 1760.

Certain of Messance's contemporaries described luxury consumption as inimical to population growth in two respects: (a) luxury production and consumption diverted men from the production of subsistence to the production of factitious goods; (b) luxury caused the standard of life to rise and thus tended to deprave the *moeurs*. The defenders of luxury reasoned that it was favorable to population growth because: (a) luxury production provided employment; (b) man's passions were such that the prospect of luxury animated him to work harder and often with more ingenuity, and thus to produce more goods. Messance accepted the first of these positive defenses of luxury, and specifically rejected the initial criticism of luxury.

Defining "luxe" as consumption in excess of that which is absolutely necessary, Messance reasoned that luxurious consumption was a source of evil only when indulged in by persons lacking "an income very superior to common needs." Luxurious consumption on part of the wealthy was "useful" in that it provided employment and subsistence to the propertyless. "It gives to non-proprietors the means of subsisting, and thereby it augments consumers, and in consequence the number of inhabitants and the sum of productions."[49]

Messance virtually denied that "the multiplication of forage and the great number of Horses diminshes the subsistence appropriate to nourish men."[50] This assertion could be true, he said, only on condition that the land used for grass and pasture could produce more "subsistence destined to nourish men" than could be attributed to the "fumiers" and the "engrais" resulting from the "consumption of the horses." Presumably this condition was not satisfied. Were not the richest agricultural yields found near cities and villages where much muck and manure were put on the soil? Were yields not feeble on land lacking muck and manure?[51] He indicated, further, that cultivation by means of horses was more economical than other modes of cultivation, and that, consequently, were the use of horses abolished, the public weal would be injured.[52]

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[49] *Ibid.*, p. 85.

[50] Mirabeau took this thesis from Cantillon and popularized it in *L'ami des hommes*. It was sometimes said that horses could depeople a kingdom.

[51] *Nouvelles recherches*, p. 85. Condillac had reasoned in a similar manner, but is not cited.

[52] *Ibid.*, p. 86. Messance believed that "calculation in the individual interest brought about the general good;" that enterpreneurs, acting in response to relative prices, produced at the lowest cost the commodities which the community desired most (*ibid.*, p. 86).

According to many eighteenth century writers urbanization brought about depravation of the *moeurs,* and "cities depeopled the countrysides." Messance replied specifically only to the latter assertion, saying that urban growth was favorable to rural growth; for cities provided markets for agricultural production. In proof he pointed out that the small towns and rural sections of north France were more peopled than were corresponding areas in middle and south France—a condition traceable to the fact that in the North there existed "a great number of cities . . . which comprise a great number of consumers." One could say, therefore, that "the cities could people the countrysides"; and that were great cities with industry to develop in middle and south France, the market for agricultural products would be increased, and the rural population would grow.[53] As is indicated below, however, Messance looked upon the rural population as the sole source of continued natural increase. Presumably he could have argued that even though city populations did not replace themselves, their presence stimulated rural population growth.

Misery, especially that of the rural population, checked population growth in France, according to almost all French writers. Although Messance did not deny this contention, he apparently considered misery to be much less of a deterrent than did most of his contemporaries. He commented upon the misery of the rural population,[54] and urged the improvement of their lot;[55] yet he implied that the misery of the poor was exaggerated. For, since misery was caused by one's having

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[53] *Ibid.*, pp. 59, 87. See also pp. 30-32, where he emphasizes the importance of establishing conditions favorable to internal commerce.

[54] He looked upon the rural population as the main source of the nation's economic and military strength (*ibid.*, pp. 28-29). "One owes to the labors of the inhabitants of the countryside subsistence and primary materials: they are the foster-fathers of the *Patrie* (*ibid.*, p. 28)."

[55] E.g. He proposed measures for the control of diseases affecting domestic animals and of plant pests; advocated the abolition of the "corvée in nature"; stressed the importance of developing roads and thus stimulating the sale of agricultureal products (*ibid.*, pp. 30-36). He favored a more equitable distribution of the tax burden (*ibid.*, pp. 58-59, 90-120), and the restriction of this burden, which rested primarily upon the rural population (*ibid.*, p. 28), to "the sum which must be expended to guard the State, conserve it, and make it prosper (*ibid.*, p. 39)." He thus made some concession to the many writers who associated the burdensomeness of taxes with their magnitude and their inequitable distribution. He did not condemn the resort of the government to the issue of *rentes viagères,* however.

more wants than means to satisfy such wants, he who had the fewest wants was most free of misery. Wherefore the misery of the peasant, who had few wants "is ordinarily more apparent than real." The peasant suffered little physically, and almost not at all morally. "Religion" and "natural philosophy" made him patient and resigned and nearly insensible to evils. Misery was to be found, on the contrary, among the urban workers who lived like bourgeoisie in good times, but felt want keenly in times of high grain prices or unemployment. Misery was to be found, too, among those who, although rich on the surface, had lost, through intemperance and idleness, health, strength, appetite, and the ability to sleep.[56] Although Messance did not relate misery to population growth, his supposition that the rural population produced an excess of births over deaths indicates that he considered rural misery inadequate to check population growth.[57]

V

Although Messance dealt only incidentally with differential fertility, he recognized that since the noble and the urban populations did not replace themselves, whereas the rural population always produced an excess of births over deaths,[58] an inter-class circulation of population inevitably resulted. This demographic metabolism, as conceived by Messance, functioned as follows: The excess rural population migrated to the cities, the ruralites becoming workers, artisans, manufacturers, or merchants.[59] The most active, economical, intelligent, and fortunate

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[56] *Ibid.*, pp. 37-38.

[57] *Ibid.*, p. 57. Service in the French militia, consisting (by the law of 1726) of 60,000 members, was frequently described as disruptive to agricultural life, and a cause of rural depopulation. Messance apparently did not share this opinion for he said that but one "garcon" in 54 needed to be taken annually for militia service. (*Ibid.*, p. 62.)

[58] *Ibid.*, pp. 57, 59-60. He states (p. 57): The experience of all time proves . . . that the source of all population is in the inhabitants of the country (campagne); it is they who have formed the other classes, and who recruit them without cessation.

[59] Messance classified the French population as follows: (1) cultivateurs, pâtres, pecheurs; (2) ouvriers, artisans; (3) fabriquans, marchands; (4) negocians, banquiers; (5) financiers; (6) magistrates; (7) militaires, les hommes de la cour; (8) clergé; (9) le souverain et sa famille. The first four classes "are occupied in nourishing, clothing, and furnishing all the needs of the nation." The last four have "the noble functions of maintaining order and peace, of making the arts

of these acquired wealth and the rank of nobility; and their children became magistrates, military men, and members of the court or of the ecclesiastical class. As some of these risen families, in turn, became extinct, even as had the noble families whom they replaced, their places were filled in turn by families ultimately sprung from the agricultural classes.[60] Whether such selection tended to be eugenic or dysgenic, Messance did not indicate.

VI

As has already been implied many French writers, in discussing the geographical distribution of the population, reasoned that too large a proportion of the population lived in the cities. Messance did not share this opinion. He estimated the density of population, 793 per square league, to range between 901 in the North and 681 in the South.[61] This variation in density he attributed in part, one may infer from his favorable comments upon the effects of urbanization, to the fact that there were relatively more cities in the North.[62] He observed too that population density tended to be greater in agricultural districts where viticulture was pursued than in areas where other crops were raised, inasmuch as viticulture required relatively more hands.[63] He estimated at slightly over ten per cent the proportion of the nation's population living in the principal cities, but did not believe this proportion to be so large as to be socially disadvantageous.[64]

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flourish, of favoring agriculture, manufactures, commerce, by great roads, canals, and a great navigation; finally, to defend the Kingdom against attacks from outside." See *ibid.*, p. 56.

[60] *Ibid.*, pp. 56-57. Messance was uncertain whether the crown ought to recruit the nobility by consciously choosing only persons of great merit, or by selecting members of the groups which had enough merit "to separate themselves from the crowd." He did not approve of the sale, by the crown, of offices conferring nobility, under conditions which shrank the nation's tax base by freeing the new noble of his obligation to pay the taille; but he did approve of the sale of offices conferring nobility on condition that one effect of such sale would be a permanent augmentation of the revenues of the state. See *ibid.*, pp. 57-60.

[61] *Ibid.*, p. 48.

[62] *Ibid.*, p. 87.

[63] *Ibid.*, p. 48.

[64] *Ibid.*, pp. 53-54.

In a larger measure than other writers, Messance discussed the determinants of industrial location with respect to Saint-Étienne where important arms and silk manufactories were located. He attributed the location of these industries in Saint-Étienne to two conditions, the lowness of wages and the lowness of the price of coal.[65] The lowness of the price of coal he traced in turn to the fact that wages were very low in the coal industry.[66]

Even though he attributed the cheapness of coal to the lowness of the wages paid to coal miners,[67] he urged that the government insure the industrial future of Saint-Étienne by taking steps to prevent the non-industrial use of coal. For were the coal supply exhausted the resource basis of Saint-Étienne would disappear. He thus clearly recognized the importance of husbanding the exhaustible resources of the community, a point apparently not again stressed by a French writer on population until after William Stanley Jevons wrote, in 1865, with respect to England's coal supply and her industrial future.[68] Moreover, in emphasizing that since the state is eternal, it must calculate in terms of the eternal and husband its resources accordingly, Messance anticipated the fascist argument[69] for state intervention in demographic

[65] *Ibid.*, pp. 121-26.

[66] *Ibid.*, p. 125.

[67] The price of coal was low because the extraction of coal was in "the hands of a class of men who know not ambition, and who limit themselves to find in this work, bread, and the most coarse clothing." At the same time, despite this explicit adherence to a kind of standard-of-living explanation of the wage level, he stated that the "low price of unskilled labor is not able to be occasioned by the low price of provisions." See *ibid.*, p. 25. What he meant apparently was, not that money wages were in no way affected by the price of provisions, but that the general wage level was low because the workers' requirements were so few.

[68] *The Coal Question*, London, 1865. L. Kerrilis, who discussed this problem a decade later, was more optimistic than Jevons. See *Journal des économistes*, 3rd ser., XLIII, 1876, pp. 198-99. Germain Garnier, in the 1822 edition of his translation of Adam Smith's *Wealth of Nations* (*Recherches sur la nature et les causes de la richesse des nations* (Guillaumin edition, Paris, 1843) pp. 194-201, note) observed that the French coal supply was not being used with sufficient economy.

[69] E.g. C. Gini reasons that the interest of the nation differs from the aggregate of the interests of the individuals composing it because the "duration of the

matters. Messance reasoned specifically that if France conserved her coal supply she would remain master of the munitions and heavy industries, and therefore master of Europe.

> The Government must not calculate as an individual; it must not see an end to its duration; it must act as if it must exist as long as nature.
>
> That the avid English carry to all nations the coal of the land which has been prodigal to them, *laissons-les-faire;* in some centuries, they will no longer be our rivals in works of iron and steel.
>
> Let us be economical with a treasure which, once dissipated, can no longer be replaced.
>
> The People that will be the last to supply its forges, will necessarily be the master; for it alone will have arms.[70]

Messance therefore proposed that steps be taken to limit the consumption of coal, since the government could not augment its supply. Let the use of coal be restricted to plants and shops actually requiring it; let it not be used in the home and elsewhere for heating, cooking, and similar purposes. To those who might assert that the supply of wood was low, and that therefore coal needed to be employed for all purposes until the wood supply could be augmented, Messance replied: If coal is employed in ordinary uses, the price of wood will fall, proprietors will not have an incentive to plant more trees, and the forests will disappear. If, on the contrary, wood is used, its price will not fall, and the planting of trees will be stimulated. Messance suggested, further, that the government might proportion wood consumption to the rate of wood reproduction, and encourage proprietors to increase the wood supply.[71]

In summary it may be said that Messance apparently was, in general, not a brilliant and imaginative social pioneer, but rather a plodding and painstaking observer and utilizer of statistical and factual materials. In his treatment of the lot of the ruralites and of the supposed functions of the upper classes he evidenced a decidedly upper-class bias. In his reply to those who looked upon horses as displacers of men, in his dis-

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lives of individuals [differs] from that of the nation." See "The Theoretical Bases of Economic Policy," *Journal of Political Economy,* XXXVII, 1929, pp. 639, 643.

[70] *Ibid.,* p. 128.

[71] *Ibid.,* pp. 127-28.

cussion of demographic metabolism, and in his defense of resource conservation, however, Messance showed an insight uncommon in the eighteenth century. And in his insistence upon the usefulness of statistical materials he was, perhaps more than any other eighteenth century writer, the true father of French demography.

STUDIES ON TWINS[1]

I. THE RELATION OF ORDER OF BIRTH AND AGE OF PARENTS TO THE FREQUENCY OF LIKE-SEXED AND UNLIKE-SEXED TWIN DELIVERIES

BY J. YERUSHALMY AND S. E. SHEERAR

BIOLOGISTS distinguish two major types of twins: the monozygotic or identical twins, where presumably a single ovum produces two offspring; and the dizygotic or fraternal, in which the two offspring result from two ova. The classification of a pair of twins into one or the other of the major types is not entirely satisfactory. Two main methods are used. One is based on physical resemblances of the two members of a pair; the other relies upon a study of the placental relations as a means of differentiating between the two types of twins.[2] This information on placental relations is not available in most cases. The reliability of either and both of these methods as an absolute criterion for classifying the two types of twins is challenged by many writers. However, there seems to be general agreement that twins of different sexes are practically always dizygotic, while twins of the same sex may be either monozygotic or dizygotic.

The most convincing evidence of the existence of the two types of twins, according to H. H. Newman(1), is obtained indirectly. The reasoning involved is as follows: If all twins were the result of the fertilization of two eggs, then the frequency of two-male, one-male and one-female, and two-female births would be approximately in the ratio of 1:2:1. Actually, the available data for a very large number of births (2) show that the ratio of the twins of various sex is more nearly 1:1:1. There is, therefore, an excess of like-sexed twins, which can best be explained by the assumption that nearly half of all such twins are monozygotic.

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[1] From Child Hygiene Studies, Division of Public Health Methods, National Institute of Health, U. S. Public Health Service, and the Division of Maternity, Infancy, and Child Hygiene, New York State Department of Health.

[2] For a detailed description of the criteria which form the basis of the two methods, see Guttmacher(4), pp. 78-79.

That the two types of twins represent two distinct manifestations of the twinning process is demonstrated by Greulich(3), who uses the physical resemblance method of classification, and by Guttmacher(4), who distinguishes between the monozygotic and dizygotic twins on the basis of a study of the placental relations. Both studies show that the age distributions of the mothers of the two types of twins differ materially. The age distribution of mothers of identical twins is not much different from that of all mothers, while mothers of fraternal twins are generally older. Similar differences are noted in regard to the distribution of the two types of twins by parity of mother.

Most studies consider the twins as an isolated group and investigate the two types of twins by comparing their percentage distribution according to the important factors involved. While this method is satisfactory in many cases, it is deficient in others because equivalent information is not given for the population of births in which these twins occurred. Furthermore, when the selection is made on the basis of physical resemblance, only those pairs of twins in which the two members have survived are included. An additional limitation of this method of study is that it is not possible to differentiate between any of a set of correlated factors. Thus, since parity and age of mother are highly correlated, it is impossible to determine from the above studies whether one or the other or both of these factors are related to the observed differences in the incidence of fraternal twins. For these reasons it was thought advisable to investigate all the twin births which occurred among all the births in a given locality at a given time.

MATERIAL AND METHOD

The present study is based principally on the live births and stillbirths which occurred in New York State, exclusive of New York City, in 1936 and 1937. The material consists of over 166,000 birth and stillbirth certificates, which include 3,660 certificates belonging to 1,830 pairs of twins. The certificates of two members of a pair of twins were matched and the analysis is based on the number of *deliveries* rather than on the number of *births*.

From information available on birth certificates it is impossible to differentiate between the two types of twins. The only distinction that can be made is whether or not the two members of a pair are of the same or of different sex. Hence, in the following analysis the twins are divided into these two major types. In addition, an approximation to

the distribution of identical and fraternal twins is attempted by Weinberg's Differential Method (5), (6). This is accomplished by subtracting from each group of like-sexed twins a number equal to that of the unlike-sexed twins in the same classification. The remainder, supposedly, is composed of identical twins. The assumption involved in this procedure is that like-sexed dizygotic twins and unlike-sexed twins do not differ materially in their frequencies with respect to the various factors studied. Comparison of results thus obtained with those of Greulich and Guttmacher may be used as an indication of the validity of this assumption.

The present study[3] deals with the frequency of twin deliveries, which is defined here as the number of twin deliveries (including those in which both members were stillborn) per 1,000 total deliveries (including those of stillbirths). More specifically, this paper deals with the relation of order of birth and age of parents to the frequency of like-sexed and unlike-sexed twin deliveries.

FREQUENCY OF TWIN DELIVERIES

One out of every 87 deliveries in the United States Birth Registration Area in the five-year period 1931-1935 was that of twins. The total number of pairs of twins delivered in the period was 126,401. At the same time there occurred a total of 10,853,459 deliveries. The frequency of twinning was therefore 11.4 per 1,000 total deliveries. In the same period there were 1,253 triplets and 19 sets of quadruplets. Thus the incidence of triplets was 1:8,662, and that of quadruplets was 1:571,235. Under the Zeleny(7) hypothesis[4] the expected incidence of triplets and quadruplets would be 1:7,569 and 1:658,503 respectively.

The 126,401 pairs of twins were distributed as follows with respect to sex: In 42,923 cases both were males; in 40,848, both were females; 42,557 cases were of unlike sex, and 73 were of unknown sex. Therefore, the ratio of the three groups of known sex was very nearly 1:1:1. The unlike-sexed twins formed 33.7 per cent of all twins. If the hypothesis is true that there are as many like-sexed dizygotic twins as there are unlike-sexed twins, then there were 41,214 pairs of identical twins and 85,114 pairs of fraternal twins. The monozygotic twins would

[3] A second paper will consider the early mortality of twins; that is, their stillbirth and neonatal mortality rates.

[4] Zeleny's hypothesis states that if the incidence of twins is 1:N, then the incidence of triplets would be $1:N^2$, and that of quadruplets $1:N^3$.

hence form 32.6 per cent of all twins. This is a higher proportion than that found by Greulich (25.4 per cent). The difference may be accounted for partly by differential mortality of the two types of twins since, as will be shown in a subsequent paper, the identical twins have higher stillbirth and neonatal mortality rates. This differential mortality may be present also in the older ages.

The total number of births that occurred in New York State, exclusive of New York City, in 1936 and 1937 amounted to 166,120, and the number of twin deliveries was 1,830. The frequency of twins, therefore, was 11.0 per 1,000 total deliveries—very nearly the same frequency as in the entire country. The 1,830 pairs of twins were distributed as follows with respect to sex: In 632 cases both were males; in 642, both were females; and in 556, one was male and one was female. The proportion of unlike-sexed twins in New York State was 30.38 per cent, which is lower than that of the country as a whole. The respective frequencies of two male, two female, and unlike-sexed twins per 1,000 total deliveries was 3.8, 3.9 and 3.3. If the hypothesis is true that there were as many like-sexed dizygotic twins as there were unlike-sexed, then the frequency of monozygotic twins was 4.3 per 1,000 total deliveries, and that of dizygotic twins, 6.7.

AGE OF MOTHER

The frequency of total twin deliveries is not uniform at all ages of mother. There are proportionately more twin births to older than there are to younger mothers. The frequency of plural births in the United States Birth Registration Area in 1935 increased progressively from a rate of 6.2 for the youngest mothers (under 20) to 18.8 for mothers 35 to 39 years old. Then there was a slight drop to 15.4 for mothers over 40. The distributions of like-sexed and unlike-sexed twins by age of mother for the entire country are not available.

The distributions of twin deliveries and of all births by age of mother and by sex of twins in New York State, exclusive of New York City, are presented in Table 1 and Figure 1.

The frequencies of total twin deliveries by age of mother in New York State was very similar to that of the entire country. The frequency was low for young mothers, increased progressively to a maximum for mothers aged 35 to 39, and dropped slightly for mothers over 40. There were distinct differences between the frequencies of the unlike-sexed and those of the like-sexed twins. The distribution of two

TABLE 1

Twin deliveries by sex and by age of mother, New York State (exclusive of New York City), 1936 and 1937

AGE OF MOTHER	NUMBER OF DELIVERIES	NUMBER OF TWIN DELIVERIES					FREQUENCY OF TWIN DELIVERIES*				
		1	2	3	4	5					
		All twins	Two males	Two females	One male, one female	Total like-sexed	All twins	Two males	Two females	One male, one female	Total like-sexed
Total	166,120	1,830	632	642	556	1,274	11.0	3.8	3.9	3.3	7.7
Under 20	14,218	88	32	35	21	67	6.2	2.3	2.5	1.5	4.7
20-24	50,018	447	164	182	101	346	8.9	3.3	3.6	2.0	6.9
25-29	46,193	527	182	180	165	362	11.4	3.9	3.9	3.6	7.8
30-34	32,230	424	138	142	144	280	13.2	4.3	4.4	4.5	8.7
35-39	17,119	267	93	84	90	177	15.6	5.4	4.9	5.3	10.3
40 and over	6,299	77	23	19	35	42	12.2	3.7	3.0	5.6	6.7
Not stated	43	...	...	...	...	...	...	..	..	..	..

* Per 1,000 total deliveries.

male twins was very much like that of two female twins. However, unlike-sexed twins differed markedly in their distribution by age of mother from either of the like-sexed twins. It will be observed that for mothers under 30 the unlike-sexed twins were generally fewer than either of the like-sexed twins, whereas for mothers over 30 they were slightly more numerous. This fact is brought out clearly in Figure 1, which presents the frequencies of unlike and like-sexed twins by age of mother. The frequency of unlike-sexed twins increased much more rapidly than that of like-sexed. Thus, mothers over 40 had more than three and one-half times as many unlike-sexed twins per 1,000 deliveries as mothers under 20, while they had only about one and one-half times as many like-sexed twins as the youngest mothers. It appears, therefore, that the frequency of like-sexed and unlike-sexed twins is differently related to age of mother.

The real difference in the frequencies of monozygotic and dizygotic twins by age of mother is obscured in Table 1, since the like-sexed twins are a composite group consisting of the two types of twins—identical

and fraternal. If we assume that the like-sexed fraternal twins have the same distribution with respect to age of mother as the unlike-sexed twins, we can then obtain the distribution of the identical twins by subtracting column 4 from column 5 in Table 1. Similarly, by doubling each figure in column 4 the distribution of the fraternal twins can be obtained. This was done in Table 2 which gives the frequencies of monozygotic and dizygotic twins by age of mother.

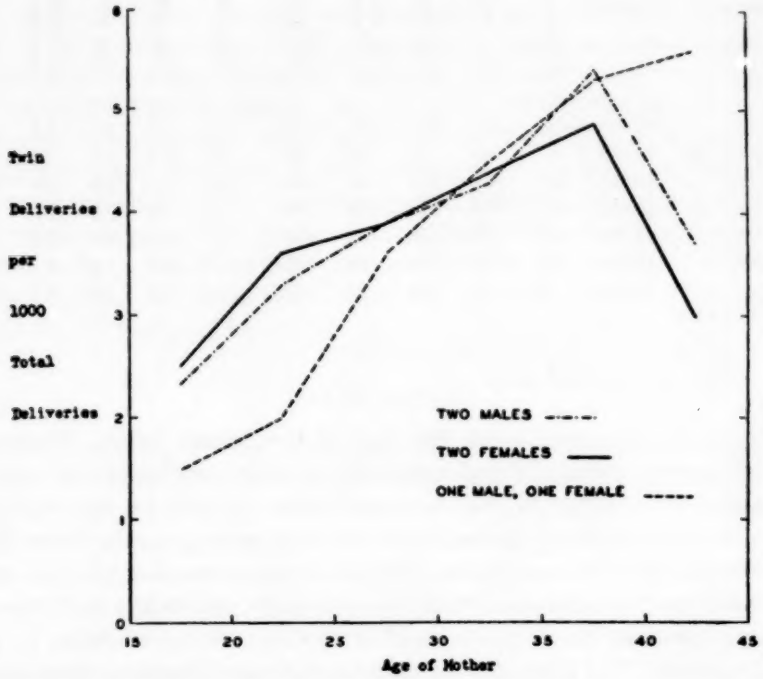

FIG. 1. FREQUENCY OF TWIN DELIVERIES PER 1,000 TOTAL DELIVERIES BY SEX OF TWINS AND BY AGE OF MOTHER, NEW YORK STATE (EXCLUSIVE OF NEW YORK CITY), 1936 AND 1937

Table 2 and the accompanying Figure 2 show that the frequency of fraternal twins increased uniformly with age of mother. The rate was 3.0 for the youngest mothers and 11.1 for the oldest. In the case of identical twins the frequency of twinning was practically constant for all ages of mother. This agrees well with the findings of Greulich and

TABLE 2

Frequency of monozygotic and dizygotic* twin deliveries by age of mother, New York State (exclusive of New York City), 1936 and 1937*

AGE OF MOTHER	NUMBER OF MONOZYGOTIC TWIN DELIVERIES	NUMBER OF DIZYGOTIC TWIN DELIVERIES	FREQUENCY OF TWIN DELIVERIES**	
			Monozygotic	Dizygotic
Under 20	46	42	3.2	3.0
20-24	245	202	4.9	4.0
25-29	197	330	4.3	7.1
30-34	136	288	4.2	8.9
35-39	87	180	5.1	10.5
40 and over	7	70	1.1	11.1

* See text for definition.
** Per 1,000 total deliveries.

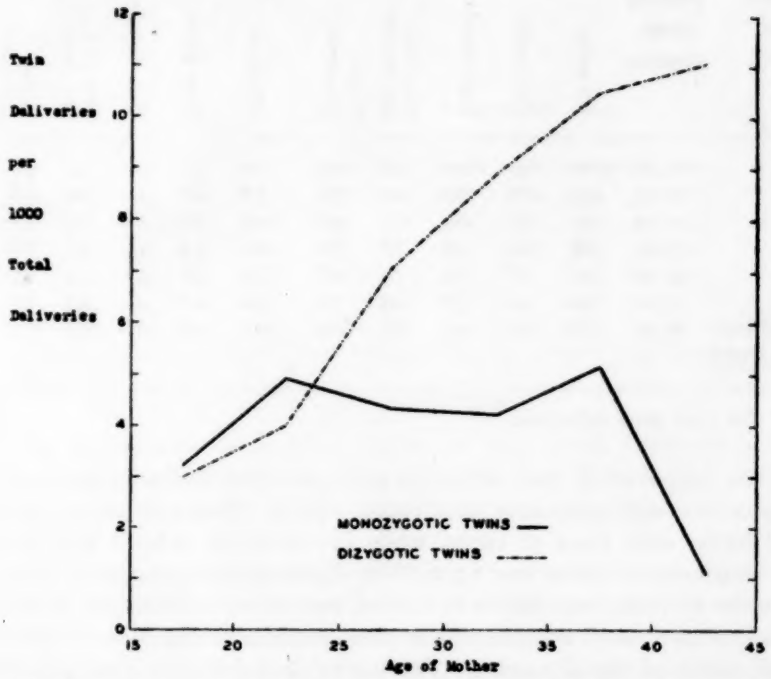

FIG. 2. FREQUENCY OF MONOZYGOTIC AND DIZYGOTIC TWIN DELIVERIES PER 1,000 TOTAL DELIVERIES BY AGE OF MOTHER, NEW YORK STATE (EXCLUSIVE OF NEW YORK CITY), 1936 AND 1937

Guttmacher and therefore adds confidence to this mode of study, and suggests that the distribution of fraternal like-sexed twins by age of mother is similar to that of unlike-sexed twins.

ORDER OF BIRTH

The distribution of twin deliveries by order of birth for the country as a whole is not available. Table 3 presents the distribution of all deliveries and of twin deliveries by order of birth and by sex of twins in New York State, exclusive of New York City, in 1936 and 1937.

TABLE 3

Twin deliveries by sex and by order of birth, New York State (exclusive of New York City), 1936 and 1937

ORDER OF BIRTH	NUMBER OF DELIVERIES	NUMBER OF TWIN DELIVERIES					FREQUENCY OF TWIN DELIVERIES*				
		All twins	Two males	Two females	One male, one female	Total like-sexed	All twins	Two males	Two females	One male, one female	Total like-sexed
Total	166,120	1,830	632	642	556	1,274	11.0	3.8	3.9	3.3	7.7
1	61,145	479	173	183	123	356	7.8	2.8	3.0	2.0	5.8
2	40,704	433	146	164	123	310	10.6	3.6	4.0	3.0	7.6
3	23,225	288	102	98	88	200	12.4	4.4	4.2	3.8	8.6
4	14,113	191	68	59	64	127	13.5	4.8	4.2	4.5	9.0
5	8,710	120	41	36	43	77	13.8	4.7	4.1	4.9	8.8
6 and over	18,194	318	101	102	115	203	17.5	5.6	5.6	6.3	11.2
Not stated	29	1	1	...	...	1	...	..	..	..	..

* Per 1,000 total deliveries.

The frequency of twin deliveries per 1,000 total deliveries increased consistently with advancing birth order. Thus, 7.8 out of every 1,000 first births were those of twins, while for births of order 6 and over the frequency of twins was 17.5. The distribution of two male twins by order of birth was similar to that of two females, while the unlike-sexed twins differed considerably in their distribution by order of birth from either of the like-sexed. For births of orders less than 4 there were fewer unlike-sexed twins than either two male or two female twins,

while for births of order 4 and over there were more unlike-sexed twins than either of the like-sexed twins. In terms of the frequency of twin deliveries per 1,000 total deliveries, the unlike-sexed twins increased much more rapidly than the like-sexed twins, as may be seen from Figure 3.

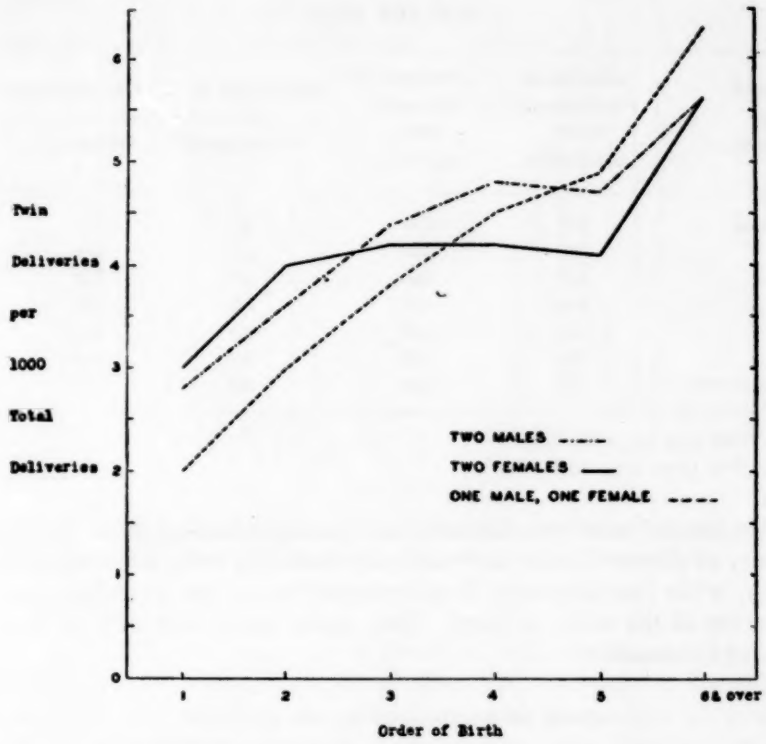

FIG. 3. FREQUENCY OF TWIN DELIVERIES PER 1,000 TOTAL DELIVERIES BY SEX OF TWINS AND BY ORDER OF BIRTH, NEW YORK STATE (EXCLUSIVE OF NEW YORK CITY), 1936 AND 1937

The frequency for the highest orders of birth was more than five times as large as that of the first births in the unlike-sexed group, while in the like-sexed twins the frequency for births of order 6 and over was less than twice that of the first births. When the figures for the unlike-sexed twins are substracted from those for the like-sexed twins, the

presumed frequencies of monozygotic and dizygotic twins are obtained. This is presented in Table 4 and Figure 4.

TABLE 4

Frequency of monozygotic and dizygotic* twin deliveries by order of birth, New York State (exclusive of New York City), 1936 and 1937*

ORDER OF BIRTH	NUMBER OF MONOZYGOTIC TWIN DELIVERIES	NUMBER OF DIZYGOTIC TWIN DELIVERIES	FREQUENCY OF TWIN DELIVERIES**	
			Monozygotic	Dizygotic
Total	718	1,112	4.3	6.7
1	233	246	3.8	4.0
2	187	246	4.6	6.0
3	112	176	4.8	7.6
4	63	128	4.5	9.1
5	34	86	3.9	9.9
6 and over	88	230	4.8	12.6

* See text for definition.
** Per 1,000 total deliveries.

As may be seen from this table and the accompanying chart, the frequency of dizygotic twins increased very markedly with advancing birth order, while the frequency of monozygotic twins was practically constant for all the orders of birth. This, again, agrees well with the findings of Guttmacher.[5]

ORDER OF BIRTH AND AGE OF MOTHER

The two factors of order of birth and age of mother are very highly correlated. It is therefore not possible to determine from the results given above whether the one or the other or both of these factors are related to the variation in the frequency of twins. In order to explore this subject in more detail, Table 5 is presented, which gives the distribution of the births and that of all the twins by order of birth and age of mother.

[5] Guttmacher gives the distribution by parity of mother rather than by order of birth, but this difference does not affect the agreement between the two findings.

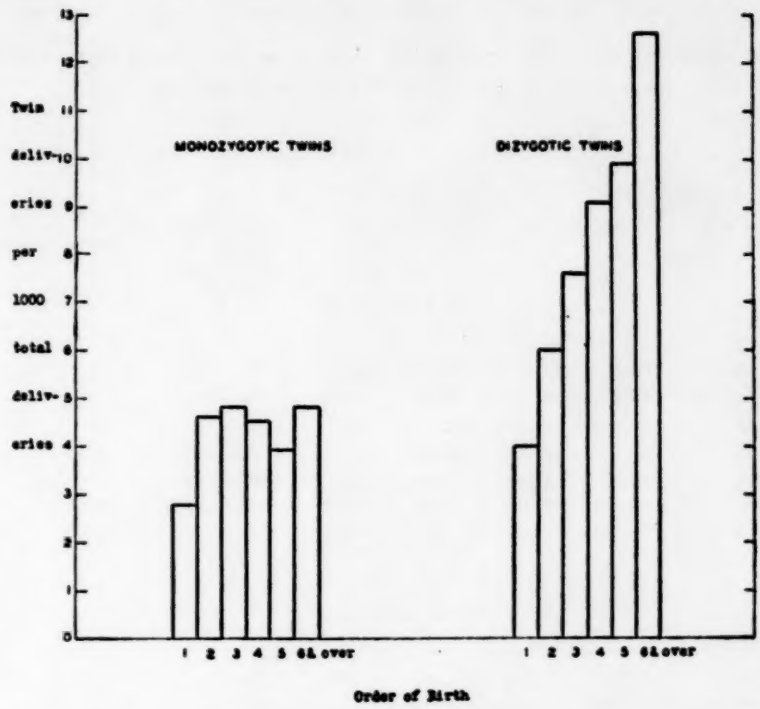

FIG. 4. FREQUENCY OF MONOZYGOTIC AND DIZYGOTIC TWIN DELIVERIES PER 1,000 TOTAL DELIVERIES BY ORDER OF BIRTH, NEW YORK STATE (EXCLUSIVE OF NEW YORK CITY), 1936 AND 1937

From Table 5 it is possible to determine the variations of the frequency of twins with either factor when the influence of the other factor is eliminated. Thus the differences in the frequencies along any of the horizontal rows are due to order of birth only, since the age of mother is constant for a given row. Similarly, the variations in the frequencies along any of the vertical columns are due to age of mother only, since the order of birth is kept constant for any column. It will be observed that the increase with order of birth was very marked for every age group of mother. The rates also increased with advancing age of mother (with the exception of the oldest age group) for most orders of birth. However, the increase by age of mother was not as pronounced as it was with order of birth. It appears, therefore, that the two factors are independently related to the frequency of twin births,

TABLE 5

Twin deliveries by order of birth and age of mother, New York State (exclusive of New York City), 1936 and 1937

AGE OF MOTHER	ORDER OF BIRTH							
	Total	1	2	3	4	5	6 and over	Not stated
Total deliveries								
Total	166,120	61,145	40,704	23,225	14,113	8,710	18,194	29
Under 20	14,218	11,483	2,384	319	32	....	....	..
20-24	50,018	26,484	14,611	5,975	2,262	686	....	..
25-29	46,193	15,464	13,431	7,816	4,650	2,710	2,122	..
30-34	32,230	5,743	7,411	5,760	4,093	2,801	6,422	..
35-39	17,119	1,661	2,460	2,744	2,384	1,878	5,990	2
40 and over	6,299	303	403	610	689	635	3,659	..
Not stated	43	7	4	1	3	....	1	27
Twin deliveries								
Total	1,830	479	433	288	191	120	318	1
Under 20	88	60	23	3	2	...	...	..
20-24	447	193	139	71	31	13	...	..
25-29	527	151	151	85	63	39	37	1
30-34	424	60	82	91	58	35	98	..
35-39	267	14	36	33	28	28	128	..
40 and over	77	1	2	5	10	10	49	..
Frequency of twin deliveries*								
Total	11.02	7.83	10.64	12.40	13.53	13.78	17.48	34.48
Under 20	6.19	5.23	9.65	9.40	62.50†	....	....	....
20-24	8.94	7.29	9.51	11.88	13.70	18.95	....	....
25-29	11.41	9.76	11.24	10.88	13.55	14.39	17.44	....
30-34	13.16	10.45	11.06	15.80	14.17	12.50	15.26	....
35-39	15.60	8.43	14.63	12.03	11.74	14.91	21.37	....
40 and over	12.22	3.30	4.96	8.20	14.51	15.75	13.39	....
Not stated	....	....	....	....	....	....	....	....

* Per 1,000 total deliveries.

† Based on less than 100 cases.

but that the frequency is more closely related to order of birth than to age of mother.

In order to bring out the difference between the like-sexed and unlike-sexed twins, Table 6 is presented, which gives the distribution of these deliveries by order of birth and age of mother.

TABLE 6

Frequency of like-sexed and unlike-sexed twin deliveries by age of mother and order of birth, New York State (exclusive of New York City), 1936 and 1937

AGE OF MOTHER	ORDER OF BIRTH											
	Number of twin deliveries						*Frequency of twin deliveries**					
	Like-sexed twins			Unlike-sexed twins			Like-sexed twins			Unlike-sexed twins		
	1	2, 3, and 4	5 and over	1	2, 3, and 4	5 and over	1	2, 3, and 4	5 and over	1	2, 3, and 4	5 and over
Under 20	42	24	..	18	3	..	3.7	8.8	..	1.6	1.1	..
20-24	156	179	12	37	62	2	5.9	7.8	17.5	1.4	2.7	2.9
25-29	110	201	50	41	98	26	7.1	7.8	10.3	2.7	3.8	5.4
30-34	40	156	84	20	75	49	7.0	9.0	9.1	3.5	4.3	5.3
35 and over	8	77	134	7	37	81	4.1	8.3	11.0	3.6	4.0	6.7

* Per 1,000 total deliveries.

Since the numbers in some cases become too small to show significant differences, the birth orders are presented in three groups: first births; second, third, and fourth births; and fifth or more births. Similarly, the last two age groups of mothers are combined in one group of 35 years and over. The rates for both the like-sexed and the unlike-sexed twins increased with advancing order of birth for nearly every age group of mother. It is noteworthy that the frequency of like-sexed twins for young mothers (under 25) increased much more rapidly than that of unlike-sexed twins of the same mothers, whereas for mothers of 25 years and over the reverse was true. When order of birth is kept

constant, it is noted that the variations with age of mother were more pronounced in the case of unlike-sexed twins than in the case of like-sexed twins.

The same method described above for the determination of identical and fraternal twins was used to obtain Table 7, which shows the assumed distribution and the frequencies of monozygotic and dizygotic twin deliveries.

TABLE 7

Frequency of monozygotic and dizygotic* twin deliveries by age of mother and order of birth, New York State (exclusive of New York City), 1936 and 1937*

AGE OF MOTHER	ORDER OF BIRTH											
	Number of twin deliveries						*Frequency of twin deliveries***					
	Monozygotic			Dizygotic			Monozygotic			Dizygotic		
	1	2, 3, and 4	5 and over	1	2, 3, and 4	5 and over	1	2, 3, and 4	5 and over	1	2, 3, and 4	5 and over
Under 20	24	21	..	36	6	..	2.1	7.7	..	3.2	2.2	..
20-24	119	117	10	74	124	4	4.5	5.1	14.6	2.8	5.4	5.8
25-29	69	103	24	82	196	52	4.5	4.0	5.0	5.4	7.6	10.8
30-34	20	81	35	40	150	98	3.5	4.7	3.8	7.0	8.6	10.6
35 and over	1	40	53	14	74	162	0.5	4.3	4.4	7.2	8.0	13.4

* See text for definition.
** Per 1,000 total deliveries.

It may be seen that the increased frequencies with advancing order of birth and age of mother were noted only in the case of dizygotic twins, while for identical twins the trend was not definite, neither with order of birth nor with age of mother. However, it will be observed that young mothers who had been delivered of a large number of previous births had a relatively high frequency of identical twins. Thus, mothers under 20 who were delivered of their second, third, and fourth births had 7.7 identical twins per 1,000 deliveries compared to a frequency of 2.1 for mothers of the same age giving birth to the first child.

Similarly, mothers 20 to 24 who gave birth to the fifth child and over had a frequency of identical twins of 14.6 compared to a frequency of identical twins of 4.5 and 5.1 for mothers of the same age who gave birth to their first, and second to fourth, child respectively.

AGE OF FATHER

That the frequency of twinning may be influenced to a certain degree by the male is indicated in several studies on the heredity of the twinning tendency (3), (8), (9). A surprising result of these investigations is the finding that the parents and siblings of the *fathers* of dizygotic twins have more twins than would be expected. Commenting on this fact, Newman (1) says, "How such a condition could be determined by the male is difficult to imagine . . . the father could hardly control double or triple ovulation in the mother" (p. 124). It was therefore

TABLE 8

Plural deliveries by age of parents United States Birth Registration Area, 1935*

AGE OF MOTHER	AGE OF FATHER										
	All Ages	Under 20	20-24	25-29	30-34	35-39	40-44	45-49	50-54	55 and over	Not stated
All ages	11.4	5.9	7.5	9.7	12.1	14.8	16.0	16.5	17.8	18.7	10.3
Under 20	6.2	5.5	6.1	6.8	5.0	5.9	9.1	13.5	7.0	...	6.1
20-24	8.7	7.6	7.9	8.9	9.4	9.5	11.3	9.2	12.9	11.0	10.8
25-29	11.6	8.7	11.2	10.9	11.7	12.7	13.1	12.0	12.5	18.0	15.5
30-34	15.2	...	9.4	14.0	14.2	15.6	15.7	18.8	18.0	24.4	16.1
35-39	18.8	...	8.5	16.7	18.6	18.1	18.2	18.9	25.9	20.6	30.1
40-44	15.4	...	...	19.8	20.3	12.1	15.4	16.1	14.2	18.1	12.4
45 and over	8.8	...	...	40.0	16.7	9.7	12.1	7.3	9.0	8.4	20.8
Not stated	9.3	60.6	9.7	3.4	4.4	5.9	...	10.2	...	...	12.0

* Per 1,000 total deliveries. Only those cases where at least 1 was a live birth.

thought advisable to investigate whether the age of the father is related to the frequencies of identical and fraternal twins.

The official publications of the U. S. Bureau of the Census (10) show the frequencies of total multiple deliveries by ages of the two parents. The figures for the year 1935 are reproduced here in Table 8.

The frequency of multiple births is seen to increase markedly with advancing age of father. Thus, for fathers under 20 years the frequency of plural births was 5.9 per 1,000 total births, while for fathers 55 and over it was more than three times as high (18.7). This increase, of course, may be entirely due to the age of mother. However, when age of mother is kept constant—that is, when the table is followed along any of the rows, it is still observed that the variation with age of father was considerable, although not to the same degree as for the total births. For example, for mothers aged 30-34 the frequency was 9.4 for

TABLE 9

Frequency of like-sexed and unlike-sexed twin deliveries by age of parents, New York State (exclusive of New York City), 1936 and 1937

AGE OF MOTHER	AGE OF FATHER															
	Number of twin deliveries								*Frequency of twin deliveries**							
	Like-sexed twins				Unlike-sexed twins				Like-sexed twins				Unlike-sexed twins			
	Under 25	25-29	30-34	35 and over	Under 25	25-29	30-34	35 and over	Under 25	25-29	30-34	35 and over	Under 25	25-29	30-34	35 and over
Under 20	38	21	1	3	15	5	..	..	4.1	6.6	4.1		1.6	1.6	..	..
20-24	109	161	43	26	40	31	20	8	6.4	7.0	6.3	9.5	2.3	1.4	2.9	2.9
25-29	14	138	143	61	7	63	64	29	7.5	7.2	8.3	7.3	3.8	3.3	3.7	3.5
30-34		17	115	141		10	45	88		6.6	8.9	9.0		3.9	3.5	5.6
35 and over		1	15	203		2	10	112		3.6	9.1	9.5		7.2	6.0	5.2

* Per 1,000 total deliveries.

fathers 20-24 and increased to 24.4 for the oldest fathers. The same trend is noted in the other rows. However, the increase with age of father was slower than with age of mother. Similar information by sex of the twins is not available for the entire country.

The distribution of the births and twin births for New York State, exclusive of New York City, by ages of the two parents was very similar to that of the entire country. Table 9 shows the frequencies of like-sexed and unlike-sexed twins by these factors.

It will be observed that the increase with age of father was marked only in the case of like-sexed twins for mothers over 30, while for unlike-sexed twins there was little change. The variation of the frequency of like-sexed twins by age of father was relatively slight. When the procedure used previously for differentiating the two types of twins

TABLE 10

Frequency of monozygotic and dizygotic* twin deliveries by age of parents, New York State (exclusive of New York City), 1936 and 1937*

AGE OF MOTHER	AGE OF FATHER															
	Number of twin deliveries								*Frequency of twin deliveries***							
	Monozygotic				Dizygotic				Monozygotic				Dizygotic			
	Under 25	25-29	30-34	35 and over	Under 25	25-29	30-34	35 and over	Under 25	25-29	30-34	35 and over	Under 25	25-29	30-34	35 and over
Under 20	23	16	4	..	3	10	..	..	2.5	5.0	4.1		3.2	3.1	..	..
20-24	69	130	23	18	80	62	40	16	4.0	5.7	3.3	6.6	4.7	2.7	5.8	5.9
25-29	7	75	79	32	14	126	128	58	3.8	3.9	4.6	3.8	7.5	6.6	7.4	7.0
30-34		7	70	53		20	90	176		2.7	5.4	3.4		7.8	7.0	11.3
35 and over		..	5	91		..	20	224		..	3.0	4.3		..	12.1	10.5

* See text for definition.

** Per 1,000 total deliveries.

is applied, Table 10 is obtained, which gives the distribution of the assumed identical and fraternal twins by age of parents.

An inspection of the rates shows that the age of the father had scarcely any effect on the incidence of dizygotic twins, while in the case of monozygotic twins there was a slight increase of frequency with the advancing age of father for nearly every age group of mother.

SUMMARY

A study is presented of all the twin births which occurred among all the births in New York State, exclusive of New York City, during 1936 and 1937. The frequency of twin deliveries was 11.0 per 1,000 total deliveries.

The relation of order of birth, age of mother, and age of father to the frequencies of twin births is investigated. These frequencies are determined separately for like-sexed and unlike-sexed twins. In addition, Weinberg's Differential Method was used in an attempt to determine the frequencies of identical and fraternal twins by the same factors. Since the results found by the method employed agree with the results of other workers who studied the problem directly, it may be concluded that the method is valuable in studying the incidence of the two types of twins.

Order of birth and age of mother were found to be related to the frequencies of fraternal twins, while in the case of identical twins the frequencies were nearly constant for all ages of mother and orders of birth.

The age of father was found to be only slightly related to the frequencies of monozygotic twins, while the frequencies of dizygotic twins did not vary consistently with age of father.

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THE CORRELATION BETWEEN INTELLIGENCE AND FERTILITY

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THERE does not seem to be any adequate information on the important demographic problem of whether there is a stable difference between the reproductivities of the intellectually better and poorer endowed elements of the population. Carefully executed studies have established that there is an inverse relation between the intelligence of children and the number of their siblings, but the inference from this premise to conclusions about differential reproductivity in the parent generation appears to be faulty. There are a few studies which have attacked the problem directly, but the samples used have been small and not representative of general populations. In view of the significance of the problem and the probably premature eugenic pronouncements that have been derived from the indirect approach, it has been thought useful to re-examine the matter with a larger and less unrepresentative population than has heretofore been used.

This population consists of those persons who left the four Providence high schools in June 1926 for whom intelligence records could be procured from the School Department and marriage and birth records from the State Bureau of Vital Statistics,[1] and whose continued residence in Rhode Island could be established from directories. Records were taken through 1938. The test used could not be satisfactorily identified, but appears to have been the same throughout the population. The principal difficulties incurred in using this group for the study of the differential fertility problem appear to be four: (1) The intelligence levels are higher than for the general population; the degree of this selection, however, is known, and proves to be lower than might be

[1] Grateful acknowledgement is made to Dr. Richard D. Allen and his staff, of the School Department, and to the staff of the Bureau of Vital Statistics, for permission to utilize the records for this research.

expected. (2) The families are incomplete; there is, however, no reason to suspect the presence of factors systematically differentiating between reproductivities before the age of 30 of persons of higher and lower intellectual status; the fraction of total reproductivity attained by this age appears to be about .43 for men and .63 for women, as estimated from the ages of parents of children born in Rhode Island in the period considered. (3) Migration is presumably a selective factor; so far as concerns intelligence, the degree of this is known, and small. (4) There is evidence of a certain degree of mistesting, in that a few scores incompatible with high school graduation occur, and in some cases retests in colleges raise the IQ's significantly; we hope that this is a random error, throwing about as many persons (and persons with about the same reproductivities) from the high to the low group as are thrown in the opposite direction.

The experimental population consists of 143 males and 230 females; the sexes differ significantly in both intelligence and reproductivity, and so will be treated separately throughout the report. The male intelligence-quotient mean and sigma are 113 and 10.3, the female 105 and 11.4. The distributions appear in Table 1. The graduates forming part of the same population, but not available for the study, included migrants, persons for whom vital statistics could not be obtained, and persons for whom no intelligence record was available. There were in the last class 32 males and 21 females, and in the others 87 males and 96 females; the latter average a trifle higher in intelligence and a trifle less variable than the experimental group (male mean 114, sigma 9.5; female mean 107, sigma 11.1); the difference reaches statistical significance in the case of the females.

The basic relation between intelligence and fertility is set forth in Table 1. The males have thus produced 62 children, or an average of 0.43 children each; and the females have produced 125 children, or 0.54 children each.

The degree of association between variables IQ and offspring number, which is the principal objective of the study, can evidently not be best measured by a correlation coefficient; perhaps the most straightforward method of measuring it is by comparing the collective reproductivity of a high-scoring with that of a low-scoring group. The maximum weight of the population size will be secured, and the effect of the mistesting error will be minimized, if we divide the groups at their

medians[2] into High and Low scoring groups; but the sense of the result is the same however (within reasonable limits) the group is divided. The following, then, is the principal result of the study, so secured: The 71 men of the High group have produced 32 children, the 71 men of the Low group 30 children; the 115 women of the High group have produced 67 children, the 115 women of the Low group 58 children. These differences are not statistically significant.

Other measures may be sought. In the High group 38 men are married,[3] in the Low group, 34; there are thus 0.84 children per husband

TABLE 1

Basic relation between intelligence and fertility

		INTELLIGENCE QUOTIENT													
		70*	75	80	85	90	95	100	105	110	115	120	125	130	135
OFFSPRING	4	..	..	..	..	..	..	..	..	..	1	..	..	..	..
	3	..	..	..	..	..	..	..	..	1	3	..	..	..	..
	2	..	..	..	..	..	1	..	4	2	2	1	..	..	..
	1	..	..	..	..	2	1	3	5	3	4	7	..	1	..
	0	..	1	1	1	4	2	10	13	22	20	11	15	2	..
	TOTAL		1	1	1	6	4	13	22	28	30	19	15	3	
	4	..	..	..	..	1	..	..	..	2	1	..	..	..	..
	3	..	..	..	..	1	..	..	1	2	1	..	..	..	..
	2	..	1	..	1	2	7	4	5	1	2	2	1	1	..
	1	1	..	..	1	2	7	10	7	3	4	3	2	..	..
	0	..	4	2	4	11	21	36	23	15	26	5	2	4	1
	TOTAL	1	5	2	6	17	35	50	36	23	34	10	5	5	1

Males / Females

* Index is lower limit of class.

[2] Male median, 114; female median, 104. In dividing the group within these IQ values, the younger subjects were assigned to the High group and the older ones to the Low group.

[3] There is no appreciable selection in intelligence for marriage in this group, or for parenthood, although the husbands and fathers are somewhat less variable than all men of the group in this trait.

in the High group and 0.88 in the Low. In the women's High group 50 are married, in the Low group, 52; the averages are thus 1.34 and 1.11 children per wife. These differences again are not significant. The men's High group have been married a total of 158 years 3 months, the Low group 157 years 3 months; the High group have therefore produced .24 children per married year, the Low group .19 children. The women's High group have been married a total of 312 years 3 months, the Low group 349 years 6 months; the averages are therefore .214 and .166 children per married year. These differences once more do not attain significance. We may conclude that from no viewpoint can a stable differential be asserted.

It is desirable to know whether this result, so contrary to the traditional belief, has been derived from a population having some accidental incompatibility with the classic ones with respect to the relationship between intelligence and sibling number. We have accordingly determined the product-moment correlation between these variables (pooling the sexes), and find it to be —.12; corrected by Kelley's formula for curtailment in range of IQ, this becomes —.17. These values are significantly large, and of the same order as has usually been found; we conclude that the usual inference from a negative intelligence - sibling number correlation to a negative intelligence - offspring number correlation is unwarranted, and that no known evidence invalidates the possibility that all the classical investigators may have studied populations in which intelligence and fertility (in the usual sense of the words) were uncorrelated.

The IQ is only insignificantly correlated with time of first (official!) reproductive activity. The *r*'s with age at marriage are: males .12, females —.03; with date of marriage, males .15, females .05; with age at birth of first child, males .00, females —.11; with date of birth of first child, males .12, females —.01.

Intelligence-fertility correlations within denominational groups (Catholic, Protestant, Jewish) and within the one identifiable nationality group (Italian), while showing occasional interesting trends, do not in any instance reach statistical significance; this is largely due to the small numbers of these subgroups having children.

The fertility differences between those who were and those who were not educated beyond high school are clearly significant (those further educated having the fewer children) for the males, but without significance for the females. The male difference lacks relationship with intelligence; the chi-square is technically significant, but is made so by

the contributions of two prolific fathers in the sub-group (of 10) with higher intelligence and without further education.

Occupations were arbitrarily classified into five groups: Professional (P); Business (B), including managers and superintendents; Skilled trades (S); White-collar (W), including clerks, salesmen, and officials of miscellaneous types; and Unskilled (U), including factory operatives, store clerks, watchmen, etc. Fertility and intelligence differences were sought with respect to occupational grouping of the parents of the subjects, of the subjects themselves, and of their spouses; also with respect to shifts of grouping between parent and self and (in the females) parent and spouse; of the former comparisons (parent occupation, own occupation, spouse occupation) the only significant relationship to offspring number is own occupation in the males; this consists of a deficiency of offspring in P and an excess in U. In this same class (own occupation, males) there is also a significant difference in occupational distribution between High and Low scorers, consisting of an excess of PBS in the High group and WU in the Low group. These facts are combined in the observation that in the Low group there is no difference in offspring number between PB and SWU (in this same male own-occupation group) but a significant one in the High group—viz., a defect of offspring in PB and an excess in SWU. This is a bit of indirect evidence for the proposition that differentials are social, i.e., occupational, rather than intellectual, in origin.

Turning to occupational shift, we find that in passing from the parental to the subjects' own generation, the B and S groups have been significantly depleted to swell the P and W groups. This has occurred with both sexes (but more markedly with the females) and without much relationship to intelligence; it apparently represents the *de facto* function of high school education. Neither the parent-subject nor the parent-spouse shift, however, appears to have a significant effect on offspring number in either sex, or in relationship to intelligence.

It is evident that the intelligence-fertility problem should be more extensively studied by direct methods.[4] Pending such studies, we feel that the above evidence, particularly since it is concordant with such

[4] To do so will evidently not be so easy as the simplicity of the necessary arrangements and the social importance of the problem might indicate. Three attempts to extend this study beyond the limits of Rhode Island have been blocked by the unwillingness of custodians of the public records to co-operate.

previous information as is available, warrants the following hypothesis: The traditional negative intelligence-fertility relationship does not exist, and, therefore, is without demographic or social significance; observed differentials are probably almost wholly of psychosocial, i.e., environmental origin, and may spring from such attitudes as overanxiety in the privileged and despair in the nonprivileged.

NOTES

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RECENT LITERATURE

USEFUL IN THE STUDY OF HUMAN BIOLOGY

In this department will be noticed the books and reprints which are currently received at the Editorial Office. Publications intended for notice here should be addressed to Dr. Raymond Pearl, Department of Biology, School of Hygiene and Public Health, 1901 East Madison Street, Baltimore, Maryland, U.S.A.

I. EVOLUTION AND GENETICS

1. *Evolution — General Principles*

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